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Magic: A Theoretical Reassessment¹

by Michael Winkelman

ANTHROPOLOGICAL THEORIES about the techniques broadly labeled magic and such related practices as sorcery, witchcraft, divination, and curing (which utilize magic) are in need of reformulation. Theoretical considerations of these phenomena have been distorted by a lack of awareness and/or acceptance of the basic assumptions of magical belief. Furthermore, the investigation and attempted explanation of magical phenomena have been seriously biased by Western cultural assumptions. Anthropologists have traditionally taken the position that beliefs about magic are empirically untenable and that there can be no such cause-and-effect relations as they imply. Theories of magic have in general tended to be rationalistic, psychological, or social-functional accounts of magical phenomena. They have rarely accepted these phenomena as reported; consequently, they have failed to consider the possibility that some magical phenomena have the empirical basis claimed by practitioners.

An impetus to the reformulation of theories of magic in the light of this possibility comes from experimental parapsychology. Laboratory research has produced empirical support for some of the phenomena claimed by magical traditions. Parapsychologists interpret their findings as evidence for extrasensory perception, or clairvoyance and telepathy, and for foreknowledge or precognition. Parapsychological research has also demonstrated that humans can exercise psychokinetic influences affecting, for example, radioactive decay and computerized random-number generators (see Stanford 1977 for review), the growth rates of plants (Grad 1963, 1964), fungi (Barry 1968), and bacteria (Tedder and Monty 1981), and healing in animals (Grad, Cadoret, and Paul 1961, Grad 1977, Watkins and Watkins 1971, Wells and Klein 1972).² Parapsychologists use the

¹ I wish to thank Joe Long, Duane Metzger, Patric Giesler, David Hess, Lola Romanucci-Ross, Robert Morris, Stanley Krippner, Berryl Bellman, Dean Sheils, and Jay Lemke for their invaluable comments on earlier drafts of this paper. Unfortunately, I have been unable to incorporate all of their suggestions. Portions of earlier drafts of this paper were presented at the 1979 American Anthropological Association meetings and the 1980 Southwest Anthropological Association meetings. A short paper on some of the problems explored here, "Magic and Parapsychology," appeared in *Phoenix* 4:7-14.

² For additional studies in parapsychology, see the *Journal of*

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term psi to refer to an assumed unitary force underlying the phenomena they study. Psi is defined in the *Journal of Parapsychology* as "a general term used to identify personal factors or processes in nature which transcend accepted laws" (1948:311) and "which are non-physical in nature" (1962:310), and it is used to cover both extrasensory perception (ESP), an "awareness of or response to an external event or influence not apprehended by sensory means" (1962:309) or inferred from sensory knowledge, and psychokinesis (PK), "the direct influence exerted on a physical system by a subject without any known intermediate energy or instrumentation" (1945:305).

Data collected by anthropologists and others suggest that some aspects of magical practice involve psi. Lang (1894, 1897), in a cross-cultural review of anecdotal and ethnographic evidence, argued for an interpretation of magical events in a cultural context which accepted the possibility of the paranormal. De Vesme (1931) recounted numerous reports of paranormal events by military officers, physicians, clergymen, colonial officials, and reporters as well as anthropologists. Barnouw (1942) examined the similarities and differences between Western mediumship and the Siberian shamanistic tradition in a context which accepted an effective paranormal aspect. He later (1946) explored the influence of culture upon the incidence and form of paranormal phenomena. Elkin (1977 [1945]) studied Australian Aborigines' reported paranormal abilities and suggested that they were related to Rhine's experimental parapsychology. Rose and Rose (1951; Rose 1955, 1956) and McElroy (1955) experimentally assessed the psi abilities of Australian Aborigines and found significant levels of ESP. Long (1977a) reviews numerous ethnographic accounts in which apparent paranormal events are reported.

The problem with many ethnographic reports of such events, as Singer and Ankenbrandt (1980) point out, is that anthropologists have failed to make clear whether these reports are their informants' own observations or beliefs, their own observations, scientifically established occurrences, or ad hoc rationalizations. However, anthropologists have recently begun to document paranormal events directly. Castaneda's (1968, 1971, 1972, 1974, 1977) reports of paranormal events have strained the credulity of many (e.g., de Mille 1976, 1980). Dobkin de Rios (1972) suggests that she herself may have demonstrated clairvoyance or precognition in the use of divination cards in fieldwork. Wedenoja (1976) carefully and thoroughly documents a poltergeist case encountered in Jamaica. Long (1977b) reports apparent paranormal events encountered with the Cumina cult in Jamaica. Sharon (1978) reports clairvoyant and precognitive occurrences associated with a Peruvian

Parapsychology, the *Journal of the American Society for Psychical Research*, the *Journal of the Society for Psychical Research*, the *European Journal of Parapsychology*, *Research in Parapsychology*, Wolman (1977), and Krippner (1977-).

curer. Romanucci-Ross (1980) reports a large number of patently paranormal events encountered in field situations on several continents. Other anthropologists may also have observed paranormal events in the field but remained silent either because of the fear of censure by their colleagues or because of the lack of an appropriate theoretical framework for their discussion.

Not only do many contemporary and historical ethnographic reports suggest a psi component to magical practices, but the implications of psi are being assessed in other areas of anthropological concern. Emerson (1977, 1979), Goodman (1977), and Jones (1977) have integrated psychics into their archeological research. My own studies (Winkelman 1979, 1981; Winkelman and Kawanami-Allen n.d.) have suggested that school experience and cognitive development affect extrasensory abilities. Hallpike's (1979:479) explorations of the implications of developmental psychology for theories of primitive thought emphasize the importance of recognition and investigation of the psi aspects of human cognition: "only when we have fully investigated the possibilities of paranormal phenomena shall we be able to claim our theories of primitive causality and spiritual beliefs are entirely objective and not distorted by the contemporary orthodoxies of our own culture."

All of this suggests that anthropologists might want to examine more systematically the idea that magic has a psi-related aspect—that some magical practices facilitate or produce empirically verifiable effects outside of the currently understood cause-and-effect processes of nature. (This aspect may or may not extend beyond the present parapsychological definition of psi to include other phenomena such as spirits; my use of the term psi here is restricted to that definition.)

Parapsychology has not specifically addressed magical phenomena. Although the relationship between parapsychological research and magical practices is not straightforward and unproblematic,³ certain findings of parapsychological research do correspond to magical beliefs and practices as anthropologists have described them.

MAGIC-PARAPSYCHOLOGY CORRESPONDENCES

CONDITIONS

Some magical practices and beliefs reported by anthropologists share certain of the conditions found through research to be conducive to psi manifestations: altered states of consciousness, visualization, positive expectation, and belief.

Altered states of consciousness. A central characteristic of magical practice is the alteration of consciousness. Mauss (1972 [1950]) points out that possession is "one of the magician's professional qualifications" (p. 39) and that magic "requires and produces an alteration, a modification in one's state of mind" (p. 128). Elkin (1977:56, 61, 62) and de Vesme (1931:205) also consider altered states of consciousness basic to the exercise of shamanic power. Similarly, Norbeck (1961:86) suggests that "falling into trance often constitutes the call to [magico-] religious vocation" and that "the ability to enter

this state is a requirement for the specialist." Mauss's observation that an unbroken focus of ritual attention is absolutely necessary if magic is to be effective underscores the importance of the state of consciousness. The yoga tradition suggests that psi abilities are by-products of spiritual development which involve direct and profound alterations of consciousness (Evans-Wentz 1978 [1935]).

Horton's (1977) review of 87 parapsychological studies indicates that a wide range of altered states and procedures (meditation, hypnosis, induced relaxation, sensory deprivation) significantly improve ESP and PK performance. Palmer's (1978) review suggests that the depth of the altered state of consciousness significantly influences the extent of the performance, other experimental factors determining whether scores are significantly above or below chance expectation. The optimal states for ESP in experimental situations are thought to include nondefensive openness to one's self and loosening of self boundaries (Osis and Bokert 1971), relaxation in which attention is directed toward internal processes, feelings, and images (Braud 1975), decreased muscular tension (Horton 1977), an abrupt increase in alpha activity (Stanford 1971), and a meditation-like state (Schmeidler 1970). All this suggests that the states of trance and possession widely associated with magical traditions might be conducive to extrasensory awareness.

Visualization. Frazer (1929) suggested that one of the basic principles of magic is the law of similarity, the belief that the magician can produce the desired effect by imitating it. The law of similarity is exemplified in such beliefs as that "like produces like" and that the treatment of the representation (image) affects the thing represented. Malinowski identified the imitation and enactment of desired ends as universal aspects of magic; one practicing magic "sees in his imagination . . . the images of the wished-for results" (1954 [1927]:80); "the ritual of magic contains usually some reference to the results to be achieved; it foreshadows them, anticipates the desired events" (1972 [1931]:68). Skorupski (1976:149) points out that the "characteristic kernel" of magic "seems to be either an impersonal present-tense description of the consequences of the magical actions, or a first-person present-tense description of what is being done in terms of the consequences." Elkin (1977 [1945]:43) suggests that the sorcerer "concentrates his mind until he sees a picture of his victim . . . and then with all the emotions and energy he can summon, he whispers 'Die!' . . . He realizes that he must 'see' the victim, visualizing the desired result." Evans-Wentz (1978 [1935]:172-209, 329-30) argues that complex visualization is one of the two fundamental processes central to the development of psychic powers.

The enactment in one's mind of the desired state of affairs has been identified experimentally as a goal-oriented strategy. Morris, Nanko, and Phillips's (1979) work, replicated by Levi (1979), compared cognitive strategies for psychokinetically affecting a light display determined by a random-number generator and found that "vividly visualizing the light being lit, e.g., the goal for the trial," was statistically more successful than the process-oriented strategy of visualizing energy flowing into the circle of lights in the desired direction. Goal-orientation is well established as underlying psychokinesis in experimental studies (Stanford 1977, Kennedy 1979). Morris (1980a) compared visualization and concentration as strategies for ESP performance and found visualization significantly better.

Positive expectation. Malinowski's discussion of the magical practitioner's anticipation of the desired result demonstrates the importance of positive expectation in the magical tradition. The trickery and sleight-of-hand used to create the appearance of psi events are also widely reported in magical practices, especially healing. This deception is often perpetrated by the magical practitioner because it is known to produce a successful outcome at another level (Rose 1956, Lévi-Strauss 1963a, Reichbart 1978). The ethnographic material on magical prac-

³ There are profound methodological and epistemological differences between parapsychology and magic. Magic is apparently a static body of knowledge, while parapsychology is an ongoing investigation. Magic assumes the observable influence of sources outside of physical control, while parapsychology employs a methodology based on the assumption of potential control of relevant variables. The paranormal phenomena of parapsychology are generally observable only in statistical deviations from normal expectation, not in the macroevents or outstanding individual performances characteristic of magic. Magical phenomena are usually created by trained practitioners, while parapsychologists usually study untrained individuals. Magical phenomena generally relate to ends desired as a part of everyday life, while parapsychological research tends to occur in laboratories isolated from the realms of normal social or personal experience.

tices reviewed by Reichbart illustrates that showmanship, trickery, and other elements are directed toward creating positive expectation and are apparently believed to create or facilitate the occurrence of paranormal phenomena.

Rhine et al. (1940) pointed out the value of enthusiasm, motivation, interest, and positive motivation on the part of the experimenter for successful parapsychological experiments. Experimental studies have shown that positive mood, attitude, and expectation are important in the manifestation of psi phenomena (see Carpenter 1977, White 1977 for reviews). Kennedy and Taddonio's (1976) review of experimenter effects in parapsychological research suggested that not only did the experimenter's personality and behavior have direct consequences, but expectations of success and friendly, highly motivating, and confidence-inspiring relationships with the subjects were vital to the success of the parapsychological experiment. Taddonio (1976) found that the subjects of college-student ESP experimenters conformed to the induced expectations of success or failure at statistically significant levels. It would appear that magical practitioners knowingly manipulate a wide range of supports which facilitate psi as well as other effects.

Belief. Mauss (1972 [1950]) noted the central role of belief in magical practice, pointing to the widely held view that the presence of nonbelievers renders magical activities null and void. Barnouw (1942) noted that shamans break off a seance when doubt is expressed. Opler (1936) reported that Apache shamans consider it impossible to cure someone who is skeptical. Shah (1968:18) recorded the cabalist view that "even the teachings themselves were actually weakened by being told to incredulous people."

Schmeidler and Murphy (1946) found that individuals who believed they would be able to manifest ESP abilities in the test situation did so to a significantly greater degree than those who did not. Palmer's (1971, 1978) reviews of experimental studies indicate that the relationship between belief in ESP and ESP performance is well established. Although the initial studies occasionally suggested a significant relationship between disbelief and negatively significant performance on ESP tasks, subsequent studies generally demonstrate a neutral performance for nonbelievers. The suggestion here that belief in the impending occurrence of a paranormal event increases the likelihood of its occurrence provides a basis for speculation upon the relative infrequency of personal experience of paranormal events in societies and cultures which generally deny the possibility of such occurrences.

THEORY

Theoretical similarities between magic and parapsychological research may be observed in (1) the concepts of psi and mana, (2) the types of phenomena which magical practitioners typically attempt to influence and those most likely influenced by psychokinesis in experimental situations, and (3) divination and the types of activities research has identified as potentially revealing psi-mediated information or the operation of psi processes.

Mana and psi. Mana, a word borrowed from the Melaneans, refers to supernatural power. Mauss (1972 [1950]), along with other early anthropologists (e.g., Marett, Preuss, and Hubert), suggested that mana is the basis of magic.⁴ Many, if not most, anthropologists (e.g., Mauss 1972 [1950], Radin 1957, Lowie 1952, Norbeck 1961) consider the mana concept universal or nearly so. Codrington (1969 [1891]) said that mana was a "force altogether distinct from physical power" (p. 118) that

was "believed by the natives to cause all such effects as transcend their conception of the regular course of nature" (p. 191). According to Mauss, mana "is a spiritual force in so far as it does not work mechanically and can produce its effects from a distance" (p. 110); "it is spread throughout the tangible world . . . and [is] ever immanent" (p. 111); "mana is also a milieu, or more exactly functions as a milieu" (p. 112). Radin (1957) cites a Maori religious thinker's description of the impersonal aspect of mana as the source of all things that possess form, growth, life, and strength. Handy (1927) suggested that mana underlies both physical and supernatural orders. Handy (1927), Radin (1957), and James (1961 [1938]) have suggested dichotomies of the mana concept that attempt to capture a mystical impersonal natural force or vital essence as opposed to a magical personal spirit or soul essence, either of which, however, may be used by humans and acquire personal or impersonal qualities.

Parapsychologists generally consider psi nonphysical and inexplicable within the currently known laws of physical science or their extension (Broad 1962, Braude 1980). Palmer's (1978) review of the basic experimental findings in parapsychology and recent research by physicists (Puthoff, Targ, and May 1980) suggest that neither time nor distance inhibits psi performance independent of the expectations of the participants.⁵ Rao (1978:283) argues that theories of psi, both physical and non-physical, "find it necessary to postulate an agency that is endowed with the ability to make direct contact with the target transcending the inhibitory effects of space and time, or a medium that makes the necessary link of contact." Rao (1979) speculates that psi is a fundamental process in nature, both involved in natural processes and mediated by the intentionality of nature's constituents (e.g., humans).

The concepts of mana and psi are far from being highly congruent. Psi lacks spirit, god, and personalized conceptions; it is characterized almost exclusively as an impersonal force, akin to electromagnetic energy. Nonetheless, the two concepts do share a number of characteristics. Both are used to refer to a non-physical power operating outside of the known physical laws or normal courses of nature and outside of normal space-time constraints. Both are at once means of action and milieux as well as fundamental processes in nature. Rao's view of psi as a basic natural process mirrors the conception of mana as both a paranormal power and the basis of the physical order. Rao (1979) and Radin (1957) offer conceptualizations of psi and mana, respectively, which exhibit complex congruences, including identical metaphors, that do not appear to be merely a consequence of approaching unknown domains.

Magic and psychokinesis. Malinowski (1954 [1927]:139-40) describes magic as a technique used to influence domains that are incalculable, unknown, uncertain, or unaccountable and not used where there is certainty, where skill or knowledge will suffice, or to replace labor. Mauss (1972 [1950]:61) characterizes magical acts as placing objects or beings "in a state so that certain movements, accidents, or phenomena will inevitably occur" or bringing them out of a dangerous state; he emphasizes the role of chance and the necessarily indeterminate nature of the outcome (pp. 81-82). Evans-Pritchard (1972 [1958]:476) says that "magic is only made to produce events which are likely to happen in any case." Elsewhere he reports that "the Azande do not ask questions to which answers are easily tested by experience . . . they only ask questions that embrace contingencies" (p. 340). Thus the common characterization of magic offered by anthropologists seems to be that it is used to influence indeterminate events, not determinate ones, and that

⁴ Malinowski originally objected that mana cannot be at the basis of magic for two reasons: (1) magic is a purely human activity, while mana is in inanimate things; (2) mana cannot be a universal concept and the basis of magic because of its wide variation. He later suggested, however, the point of view held here: "every magical ceremony is, in its essence, a handling of mana" (1935, vol. 2:68).

⁵ Since psi seems to operate independently of space and time, it has been assumed to be nonphysical. However, the range and characteristics of phenomena affected experimentally through psychokinesis suggest physical limitations on psi and perhaps a quasi-physical nature (see Stanford 1977).

notions of chance, contingency, and indeterminacy are central to its nature.

One of the principal early findings of PK research was that systems with a greater degree of randomness were more easily influenced (Forwald 1954, Cox et al. 1966). Kennedy (1979:291) suggests that "psi can operate when there is . . . a situation in which an element of randomness or indeterminacy is involved." Stanford (1977:369) points out that "PK can readily influence systems in a transitional or indeterminate phase between states" and that "changes in the state of the physical system may in some sense be opportunities for PK influence to occur." Braud (1980:301) reports on a series of experimental studies supporting the notion that the lability of a system, "the ease with which a system can change from one state to another and the amount of 'free variability' in the system," is positively related to the ability to influence it psychokinetically.

In both parapsychology and magic, then, the domains of effective intervention are those in which chance has a wide range of possibilities for affecting the outcome.

Divination and psi-mediated information gathering. Divination entails acquiring information about events beyond that which is available through inference or ordinary sensory means. Divination is probably present in every human society (Wallace 1966). Wallace suggests that divination functions as a mechanical device for choosing among equally preferable alternative solutions when available information does not permit informed decision or for executing inoffensive group decisions when there is disagreement among members. He also points out, however, that divination serves as a randomizing device and speculates (in agreement with Malinowski) that divination is used "in precisely those areas or on those occasions of human endeavor where empirical or scientific knowledge is least adequate and where uncertainty most prevails" (p. 173).

There appear to be two main types of divination. One type primarily involves an altered state of consciousness (possession, trance, mediumship, drug [hallucinogenic] intoxication, dreams) in which an individual (usually gifted) interprets his experiences in the light of the question being asked. In the other type, an indeterminate and random event (e.g., object arrangement [from the throwing of bones or other objects], bone cracking [scapulimancy], animal life or death [benge], entrail arrangement, card placement and order [tarot], number of objects in randomly arranged groups [sikidy, *I Ching*], or ritually observed natural event [e.g., animal movement], etc.) is interpreted in a symbolic framework to yield information relevant to a problematic state of affairs. The indeterminacy involved in the outcomes of these events would seem to make them amenable to psychokinetic influences. PK may be the means by which the interested parties induce in the divination medium the structure of relationships which reveals the information desired. Experiments suggest the concomitant operation of ESP and PK in a goal-oriented manner (see Stanford 1977, Kennedy 1978 for review), as would be necessary for psi intervention in such situations.⁶

Some investigation of divination has been undertaken within parapsychology. Rubin and Honorton (1972) have demonstrated that individuals' ability to choose the *I Ching* hexagram that has been generated for them varies significantly with their belief in ESP. Stanford (1972) has found a significant interaction between suggestibility and success at a divination task. The potential efficacy of the procedures underlying randomization of materials is supported by experimental research on what has been called the "psychic shuffle." Rhine, Smith, and Wood-

ruff (1938) demonstrated that individuals could blindly shuffle a deck of cards and stop at points where the then blindly ordered sequence matched that of a random target deck. Such research suggests that humans can use psi to order an indeterminate aspect of nature.

DISCUSSION

This comparison of parapsychological research findings with aspects of magical belief and practice suggests substantial parallels between the two systems. Parapsychological research is of little use in determining the empirical basis, if any, of such elements of magical systems as spirits, malevolent paranormal effects, or paranormal influence on human behavior or natural systems. We may, however, be able to establish, on the basis of their similarities to principles established in experimental parapsychology, which aspects of magical practices may involve psi.

The anthropological tendency to combine magical and religious phenomena (see, e.g., Norbeck 1961) has led to the integration of a number of magico-religious beliefs and practices which resemble each other structurally but not functionally or ontologically. To establish which aspects of magic may potentially involve psi, it will be necessary to distinguish these phenomena. Malinowski (1954 [1927]:81; 1972 [1931]:69) distinguishes between (1) the spontaneous emotional forms, overflowing with passion and desire, which form the basis of magic and (2) the institutionalized traditional mythological forms embodying social values and customs into which systems of magic develop. Norbeck (1961:47) emphasizes the same distinction, pointing out that "the foundations of magical practice are . . . due to experiences actually lived through. . . these formulas become stripped of the emotion which surrounded them at their birth and are transformed into the prosaic and essentially emotionless acts which characterize most of magic." Parapsychological field studies and experimental research suggest that it is the spontaneous emotional forms of magic that are psi-related. The parapsychological tradition in survey research (see Rogo 1975 for review) has implicated emotional ties as a key to understanding the structure of spontaneous paranormal events. Spontaneous paranormal experiences generally occur during dreams, in response to strong emotional experiences of others, particularly accident or death, and between members of the same family or close friends. Parapsychological field studies of recurrent spontaneous psychokinesis (commonly referred to as poltergeist phenomena) suggest that a strong psychodynamic of repression underlies such phenomena and that the focal individual (assumed to be responsible for the phenomena) invariably has strong physical or emotional dysfunctions (Roll 1972, 1978). Parapsychological research within the psychiatric tradition also implicates emotional tension as central to psi; Ullman (1977) points out that as people near psychotic breaks or crises they manifest much stronger psi events in the course of therapy. Experiments (e.g., Dean and Nash 1967 and others reviewed by Palmer 1978) suggest that the functioning of psi is closely related to emotional factors relevant to the individual's psychodynamics.

Malinowski suggests that magic arises from spontaneous ideas and reactions (1954 [1927]:78-79) when the rational processes and known means of resolving problems have been exhausted; this implicates unconscious (or primary) thought processes as basic to magic. Experimental research suggests that psi processes are closely related to primary thought processes. According to Rhine (1977), psi processes are among the least conscious of all mental processes. Research has consistently suggested that psi information mainly involves primary processing capacities, revealing information such as shape, form, and color, as opposed to secondary processes involving analytical concepts, functions, and names (Targ and Puthoff 1977, Put-

⁶ It would appear that an immense amount of information, available only extrasensorily, would be necessary to direct PK effects into the divination medium in such a way as to reveal the desired information. However, the goal-oriented nature of psi events makes this irrelevant; ESP "guides" the PK efficiently regardless of task complexity (see Stanford 1977; Kennedy 1978, 1979).

hoff, Targ, and May 1980). My own research (1979, 1981; Winkelman and Kawanami-Allen n.d.) and research on altered states of consciousness in general suggest that higher-order analytical processes inhibit extrasensory awareness. Stanford's (1973, 1974) and Kreidler and Kreidler's (1972, 1973) research has suggested that psi processes integrate information into behavior without conscious awareness.

The congruence of these characteristics of experimental and spontaneous psi with Malinowski's characterization of the ontological basis of magic suggests that this aspect involves psi. In contrast, the institutionalized mythological forms do not have any readily identifiable correspondences to psi processes. This would suggest that we can consider nonparanormal magical practices that embody clear social or organized religious elements, rituals that serve to reinforce the social hierarchy (Skorupski 1976) such as interaction ceremonies, commemorative acts, or operative ceremonies that initiate new social status or obligations, and sacramental or other acts involving non-observable changes of state such as baptism, consecration, and ritual cleansings.⁷

Norbeck (1961) subdivides magico-religious phenomena three ways, in terms of a personified supernatural power, an impersonal power (mana) manipulated by humans, and "a conception of supernatural efficacy or power inherent in certain mechanical cause-and-effect sequences" (p. 49). He suggests that these mechanical procedures bear no direct relation to the emotions or other aspects of humans. Pointing out that some magical formulas are characterized by meticulous performance of certain steps leading to a guaranteed result, he suggests that although these mechanical cause-and-effect sequences are construed as supernatural means of accomplishing highly specific ends they have no readily observable connection with conceptions of the supernatural as a personified or impersonal power; rather, they have much in common with the cause-and-effect sequences associated with naturalism and science and with the ideas which flow from the basic mental processes. The finding that psychokinesis more likely affects random, indeterminate, variable, and labile systems suggests that although these kinds of events are similar to and well integrated with other kinds of magical phenomena they probably tend not to involve psi effects. (However, some magic so characterized may facilitate psi effects through visualization or through placebo effects.)

Titiev (1972) distinguished between calendrical and critical rites. Calendrical rites are those which take place recurrently and are tied to natural cycles, particular dates, and socially defined occasions; they "are designed primarily to strengthen the bonds of cohesion that hold together all of societies' members" (p. 433). These appear to correspond to Malinowski's traditional, standardized forms of magic and probably do not involve psi. Critical rites are those celebrated intermittently and generally only when a personal crisis has occurred. These services tend to be rendered by seers, diviners, fortune-tellers, shamans, and medicine men; they need not be performed by socially sanctioned priests. These appear more likely to involve psi.

Wallace (1966) suggests that five categories of transformation of state partition the aims of ritual: technology, therapy and antitherapy, social control, salvation, and revitalization. As a first approximation, the latter three categories may be considered, because of their social functions, not to involve psi.

Technological rituals, according to Wallace, can be subdivided into three main kinds: divination, designed to extract from nature information vital to important concerns; hunting and agricultural rites of intensification; and protective rituals, intended to prevent or avoid illness or death. Protective rituals

may be excluded from the category of psi-related magic because of the lack of directly observable consequences. Divination procedures, though they tend to acquire socially standardized forms, would be included in psi-related magic because of their involvement with emotional concerns and personal crises.

Rites of intensification mobilize, focus, and intensify natural processes relevant to success in hunting, herding, or agriculture. Some of these rites exhibit characteristics that would classify them with the standard, traditional forms of magic (e.g., calendrical rites, cause-and-effect sequences, social as opposed to individual functions), while others exhibit features that would classify them with the ontologically prior forms of magic (e.g., reference to basic emotional and physiological needs [food], techniques for influencing indeterminate states of affairs, crises). It remains to be seen if controls can be applied to determine whether or not rites of intensification involve psi.

Therapeutic and antitherapeutic rituals, pertaining largely to the cure and cause of illness, respectively, undoubtedly involve basic emotional states and relate to the ontological forms of magic. Although these rituals may also involve changing social relationships and community participation and support, they seem basically psi-related. Wallace has observed that healers (shamans) find minimal benefit in standard rituals and instead depend on dramatic and radical experiences in addition to the use of herbs, physical treatment, and psychodynamic manipulations. Although we should be cautious about seeing psi processes in curing because of the wide range of psychophysiological mechanisms which can cause healing or death (e.g., see Cannon 1942, Lester 1972, Lex 1974, Halifax-Grof 1974, Goodman 1974, Long 1977c), research suggests that psi effects may be present in many curing rituals, since human psychokinetic influence can operate on a wide range of biological systems (Grad 1963, 1964, 1977; Barry 1968; Watkins and Watkins 1971; Wells and Klein 1972; Tedder and Monty 1981).

While the distinctions just reviewed can indicate a likelihood that certain magical practices will involve psi effects, individual criteria cannot always unambiguously suggest the potential for psi. For instance, although social elements are central to the standard traditional forms of magic, the presence of social elements cannot be the criterion for eliminating a magical practice from the category of psi-related magic. Divination about social events would include prominent social elements, suggesting a non-psi process, but psi processes could still be involved through randomization techniques or altered states of consciousness. Furthermore, some aspects of magical practice, although apparently non-psi-related, may create a context which facilitates psi effects. For example, some institutionalized rituals undoubtedly help to focus attention or foster particular cognitive processes that facilitate psi effects. Thus, to determine whether a given magical activity may involve psi it will be best to apply several criteria simultaneously. For example, a commemorative rite involving mechanical cause-and-effect procedures most likely would not involve psi, whereas a healing ceremony involving altered states of consciousness and strong emotion may involve psi even though it is performed on a specified day and involves social elements and mythological references.

Table 1 provides a weak-ordered feature analysis that may distinguish magic likely to involve psi from the rest of magic. These descriptors are not exclusive attributes of either category, but they appear to be primarily descriptive of one or the other. Although these distinctions are not based upon magical practitioners' differentiation of magical phenomena and do not address all of the basic assumptions made by those practitioners, they may be useful in suggesting which areas of magical practice to examine for psi effects.

⁷ Ritual cleansings might, however, induce altered states of consciousness which could facilitate paranormal occurrences.

TABLE 1

CHARACTERISTICS OF PSI-RELATED AND NON-PSI-RELATED MAGIC	
PSI-RELATED MAGIC	NON-PSI-RELATED MAGIC
Individual performance	Group performance
Optional acts	Mandatory acts and taboos
Activity oriented to individual needs	Activity oriented to group needs
Powers commanded	Powers supplicated
Powers acquired through inspiration, ordeal, and personal experience	Powers acquired through social rites and recognition or as a gift
All members of society eligible to develop power	Power inherited or restricted to a particular social class
Secret and esoteric	Public
Critical	Calendrical
Randomizing procedures	Cause-and-effect sequence
Indeterminate outcome	Highly determinate outcome
Emotional	Rational

THEORETICAL PERSPECTIVES

An adequate theory of magic must explain the role of psi in the development of magic and the reasons for and the process of generalization of the basic structures of magic into its social forms. It should account for Mauss's observations that magic is poorly organized, lacks an institutionalized structure, and is characterized by common forms but with highly variable content. It should also address the characteristics of magic identified by other theory builders.

Malinowski (1954 [1927]:70–78) suggested that magic is universally characterized by dramatic emotional expression, imitation and enactment of desired ends, the conferral of powers on material objects, the use of formulas or spells, phonetic symbolism, evocation of states by words, and mythological elements. He held that magic arises out of emotional tension and the spontaneous flow of ideas and has had numerous independent origins. It is used, he argued, in connection with things that are of vital interest to humans but elude normal technological and rational efforts. Its efficacy depends on unmodified transmission from generation to generation.

Magic everywhere appears to utilize a concept of energy such as mana to realize its ends. Souls and/or spirits play an important role in magical activities and effects. The belief that an object that has been in contact with a person contains something of that person that can be used to cause negative effects (exuvial magic) also has a worldwide distribution. Also widely distributed are beliefs in an association between animal spirits and sorcerers' powers, the possibility of using supernatural powers malevolently, divination, telepathic communication, and out-of-body experiences, as well as such features, mentioned earlier, as the use of goal-oriented imagery and altered states of consciousness. Although not all of these factors pertain to psi-related magic, an adequate theory should be able to address their development.

Tylor (1948 [1871]) and Frazer (1929 [1911]) suggested that magical beliefs arose out of a need to understand and control the environment and are supported entirely by trickery, self-deception, and blocks to falsifiability. One difficulty with this perspective, referred to by Skorupski (1976) as the intellectualist tradition, is that it suggests we assume that magic is derived from cosmology, a domain that is highly variable and strongly influenced by social factors and therefore unable to account for the universal structure of magic. A further difficulty is with the assumption that the universal structure of magic, derived from cosmology, was extended into the experiential domain to explain psi-related experience. The similarity of the principles of psi and those of magic suggests that the exten-

sion was actually from the other direction, that is, from the known basis in human experience with psi to the unknown cosmological domain. Dwyer's (1979) work suggests that the less familiar cosmological domain would not be used to explain the more familiar one of personal psi experience.

Frazer (1929 [1911]) and Malinowski (1954 [1927]) offered universal psychological mechanisms as an explanation of the universal structure of magic, arguing that the practical rituals (Frazer) or the spontaneous basis (Malinowski) of magic exhibit universal structures because of universal mental constructs and processes. According to Frazer (p. 49), magic arose from the "misapplication of one or the other of two great laws of thought," the association of ideas by similarity and by contiguity in space or time, providing the basis for sympathetic and contagious magic respectively. The uniformity of the structure of human minds is assumed to result in similar responses to innate or elementary ideas in all peoples. This notion, however, explains only a few of the universal characteristics of magic.

Contrasting with intellectualist theories are what Skorupski (1976) calls symbolist approaches. These diverse and occasionally contradictory perspectives, linked to Durkheim and the functionalist school, share a view of magic as a symbolic system which describes the pattern of social relations in the society in which it exists. Magical ritual and belief are seen as operating together in the maintenance of the social order, ritual representing an invariant substratum of the social order and beliefs being no more than rationalizations of this substratum. Skorupski suggests that a fundamental assumption of symbolist theories is that what lies at the heart of magic is belief in the controlling power of metaphor, the capacity of symbolic enactment to bring about the event desired. Swanson (1960) uses societies selected from the World Ethnographic Sample to test whether social variables predict the presence of monotheism, polytheism, and beliefs in ancestral spirits, reincarnation, the immanence of the soul, the prevalence of witchcraft, and morally concerned gods, and his study is generally statistically successful. Similarly, Bourguignon (1979) shows that trance and possession are significantly associated with simpler and more complex societies respectively. However, symbolist approaches cannot account for universal magical practices and beliefs because universals cannot be explained as functions of highly heterogeneous social variables.⁸ These approaches explain differences in the distribution of types of phenomena related to magic at the level of its social ritual forms, not its ontological basis.

Existing anthropological theories of magic seem to be addressing, from different perspectives, one main point: the pervasive presence of certain common principles of magic throughout the cosmology and social order. The differences between intellectualist and symbolist perspectives suggest that we must explain how the processes at the psychological level are related to the integration of magical principles at the social and cosmological levels. The work of anthropologists and others who have focused upon the role of metaphor in culture (Black 1962, Fernandez 1974, Sapir and Crocker 1977, Dwyer 1979) provides us with a basis for understanding how magic, with its origin in spontaneous emotional experience, was generalized to and integrated into social and metaphysical domains.

Fernandez suggests that the sign images that link domains may come either from the primary experience of corporeal life

⁸ They may, however, still be related to universal social processes, as Swanson suggests. He hypothesizes that mana is the product of primordial links and that spirits are the externalization of constitutional structures. One application of these hypotheses to the notions developed here is that the socialization of the mind through the effects of the primordial links and the constitutional structures may collectively channel psi processes unconsciously in certain ways. The interaction of social processes and psi may project psi abilities in such a way as to create the perception of independent entities or phenomena, such as spirits or archetypes.

or from the external structural world, primarily the society of others. Magic, in arising in spontaneous emotional experience, would have involved primary thought processes, not elaborated verbalizations, and lacked means for explication and social expression. Based in primary experience, it could only have been expressed at the social level by adopting social forms. The socialization of magic would have been essentially the process Fernandez suggests in which the religious metaphors recast the inchoate and ineffable whole of primary experience in terms of experience from more manageable domains into which some aspect of that primary experience can be extended. The intimate relationship which Fernandez points out exists between the principles of sympathetic magic and the processes of metaphor and simile suggests that such a process was involved in the development of the complex of magical phenomena. (Malinowski [1972 (1931):64] also suggests that the ontological forms of magic borrowed social elements as a means of explication, but he does not develop this idea.) Following Fernandez's line of thought, the metaphoric predication of sign images from the social domain upon the inchoate experiences of magic would have concretized and socialized this internal experience and provided it with a medium for communication. Social and religious dogmas would have been imposed upon it. For example, many mystics have common phenomenological experiences, such as the perception of a dazzling white light, but their descriptions and explanations vary (e.g., angels, saints, spirits, spiritual masters, etc.).

Fernandez argues that the most effective metaphors extend the social into the corporeal and vice versa; not only does the predication of metaphor explicate magic, but it links the magical and social domains and incorporates social experience into the experiential forms of magic. This two-way metaphoric process, also noted in Sapir and Crocker (1977), involves analogical modeling. An analogue model is an "object, system or process designed to reproduce as faithfully as possible in some new medium the structure or web of relationships in the original" (Black 1962:222). The aim of analogue models is to reproduce the structure; the identity of structure is compatible with the widest variety of content. Dwyer (1979) points out that analogue models are used to extend cultural meaning when they condition comparable perception in fellow humans. Magic is a collective representation (Mauss 1972 [1950]). Hallpike (1979) points out that collective symbolic representations derive from common experiences whose private symbolism can therefore be used as a basis for communication. The universal principles of magic, arising out of the associational structures of the mind and the psi-related ontological basis of magic, provided the analogical model that was extended into the social realm in the development of the complex of magical phenomena.

This extension would have been motivated by an application of the laws of association in everyday life. As manifested in the principle of sympathetic magic, the model would probably have received empirical support from psi and non-psi successes realized by the goal-oriented guidance of thought processes. These successes would have supported the application of the laws of association in other domains, since experiences with the efficacy of these principles would have been widespread and would have evoked comparable empathetic responses. All systems of knowledge, science included (Leatherdale 1974), expand by the extension of metaphors and analogue models from known to unknown domains. Examination of traditional culture and magical beliefs and practices suggests that healing was an important area of application of the analogue model. For example, Frazer (1929 [1911]:15-16) recounts a cure for jaundice (characterized by yellow skin) in which the person is ritually cured by symbolically transferring the yellow color to yellow creatures and things through application of the laws of association, similarity, and contagion. Healing through sympathetic magic undoubtedly encountered some success through

psychosomatic and placebo responses supported by belief, as Lévi-Strauss's (1963b) account of a healing ceremony illustrates. These ceremonies undoubtedly evoke responses which induce physiological changes and cures, although not through the functioning of psi as we now understand it.⁹

The integration of magic with traditional mythological elements, social processes, and ideology would have been motivated by several factors in addition to the contiguity established by the metaphoric/analogic processes. A human tendency to integrate all aspects of the social/cultural order with common themes has been noted by Bateson (1958), Radcliffe-Brown (1965), Griaule (1965), and Lévi-Strauss (1963c). The presence of cause-and-effect-type sequences involving sympathetic magic in cosmological beliefs may be the result of the use of models provided by basic experience with associational and psi processes to explicate domains of which humans had no knowledge and over which they exercised no control. The integration of the structures of the ontologically prior forms of magic into the social domain would have provided legitimation for social structure, as Malinowski and the anthropologists of the symbolist tradition point out. Since the principles of magic received empirical support from the more basic areas of human experience, their integration into the social order would support social institutions by integrating the structure of primordial experience with cosmological beliefs and social experience. The perspective taken here suggests that the (symbolist) observations and theoretical generalizations made about magic as a social ritual are generally correct for that aspect of magical belief and are concerned with explicating the integration of the analogue models provided by the ontological base of magic into social institutions. The problem with these theories is a mistaken ontology which assumes that magical beliefs arose out of a need to express symbolically the pattern of social relations.

The processes of metaphorical predication and analogical modeling do not account for all of the actual elements, practices, and beliefs associated with magic; however, they do integrate the main concerns of earlier anthropological approaches to magic and explain how basic psychological structures and processes participated in the integration of the principles of magic into other domains. The notion of metaphorical processes based in a psychological model which includes psi aspects accounts for Mauss's principal observations on magic and for most of Malinowski's. Apparently anomalous magical universals such as belief in the power of words and spells may be reversals from the social ritual domain, where belief in the power of words may derive from social authority. However, the use of mantras among the yoga traditions suggests that we should empirically investigate the effect. Similarly, the principle of contagion should receive further investigation, since some research suggests a psi basis for its reported effects (Emerson 1977, 1979; Jones 1979; Rogo 1975).

The explication of the different aspects of social ritual magic and their relationship to the ontological forms is still insufficient, and the elements of magical belief and the principles discovered in parapsychological research have been insufficiently explored to suggest exactly which aspects of magical practice involve psi. Before we can postulate universal psychological or social structures as the basis for any magical beliefs, we must

⁹ As I have suggested, one of the problems in establishing psi-related healing is the elimination of the possibility of physiological changes as a result of psychosomatic or placebo effects. The relationship between the psychosomatic and psi is insufficiently explored. The Cartesian dualism upon which the definition of "psychosomatic" is based assumes that the mind is restricted to and dependent upon its body. In undermining these assumptions, parapsychology requires a reconciliation with the psychosomatic domain which may eventually largely eliminate the differences between the two.

experimentally investigate the phenomena in question to determine whether or not psi or related phenomena may provide an empirical basis for such beliefs.

SUMMARY AND CONCLUSIONS

The correspondences between parapsychological research findings and anthropological reports of magical phenomena reviewed here suggest that magic is associated with an order of the universe which, although investigated empirically within parapsychology, is outside of the understanding of the Western scientific framework. The congruences between some aspects of magic and parapsychology, combined with the considerations discussed above on the role of metaphoric processes in the development of the magico-religious complex, have been used here to organize a theoretical perspective on the nature of magical phenomena.

An integration of anthropological observations on magical phenomena with the findings and theoretical perspectives of experimental parapsychology suggests that the complex of magical phenomena had its origins in innate universal human potentials closely associated with psi abilities. Malinowski suggested that magic is based in laws of association inherent in human cognitive processes and exemplified in the laws of similarity. The close structural relationship of these principles to the goal-oriented principles of psi established in parapsychology suggests that the basis for the development of magic was not merely a (mis)application of basic laws of thought, but an application and extension of psi-related principles. The characteristics of psi phenomena also support Malinowski's suggestion that magic arose out of spontaneous emotional experiences and tensions. The model offered here is basically an extension of the previous psychological model which includes psi abilities—human capacities, still little understood, for affecting the world in a manner which is beyond our current understanding of the laws of nature.

Parapsychological research findings that altered states of consciousness, belief, goal-oriented visualization, positive expectation, and emotional factors are psi-conducive suggest that magical ritual was initially developed to elicit the experiences and to create the emotional states, states of consciousness, foci of attention, cognitive processing, and expectation and belief that facilitated or were believed to facilitate psi influences on human affairs. Magic adopts social forms through metaphorical predication and analogical modeling. This involves the extension of the structure (form) of the ontological basis of magic, providing the basis for the integration of a variety of structurally isomorphic but functionally different phenomena. The integration of magical principles into other aspects of culture was motivated by the perception of their effectiveness in paranormal and other psychological, psychosomatic, and social domains and the desire to use them to bring control and certainty to cosmological domains and by the need to legitimate social relations by integrating social experience with principles basic to emotional experience.

The phenomena labeled magic may be seen as fulfilling different functions with separate ontologies which have interacted and degenerated to their present structurally similar forms. At the basis is psi-related magic—techniques used to direct psi influences into human affairs. The other primary aspects of magic are the social ritual and the metaphysical or cosmological, the concerns of the symbolist and intellectualist theoretical traditions respectively. Metaphysical magical beliefs correspond to Frazer's category of theoretical magic, rules which determine the sequence of events throughout the world. They are based on an extension of the analogue model provided by the structures of universal human thought processes (association, contagion, and similarity) and psi-related principles that form the basis of

magic. This model was extended to unknown domains of human concern in an attempt to extend the possibility of control and understanding provided by these processes to domains in which humans could exercise no control and had little understanding. Social-ritual magical beliefs and practices are in part an attempt to legitimate the structure of social relationships by integrating them into the model provided by magic in primordial experience and through the manipulation of symbolic processes and symbols basic to consciousness. The integration of the analogue model provided by the ontological forms of magic into the metaphysical domain and social relations was motivated by the human tendency to integrate common themes through all aspects of culture. Most of the observations made about magic by anthropologists associated with the perspectives broadly labeled symbolist represent the product of the integration of magical principles and beliefs into social structures and beliefs and constitute what I have labeled social ritual magic. The primary limitation of the symbolists is in assuming that the need for social symbolic expression provides the basis of magic and that social ritual magic constitutes the extent of magical practice and concern.

If magical practices and beliefs seem largely non-psi-related today, it may be because of a decline in the effectiveness of psi-related practices as a result of the disruption of the social institutions that once trained practitioners to manipulate psi potentials. Rituals would have continued to be performed in an attempt to affect personal and natural processes potentially under the influence of magic, but lack of skill and change in the social, psychological, and physical environment would have led to failure and the degeneration of the psi-related forms of magic toward the social ritual and metaphysical forms with which they shared structure and common elements. Our cultural disbelief in the psi-related aspects of magic created an inability to differentiate functionally different forms of magic with a common structure.

Current anthropological theories of magic are inadequate to explain anthropological accounts of magic and related phenomena; perspectives need to be developed which incorporate the basic principles accepted by those who believe in and practice magic. The correspondences of magical beliefs and practices with experimental findings and theoretical perspectives in parapsychology suggest that some aspects of magic are related to psi, among them altered states of consciousness, goal-oriented visualization strategies, divination procedures, and influences upon indeterminate systems in transitional states. These characteristics have been used here as criteria in reviewing previous anthropological discussions of magic to show that the differentiations tend to separate magical practices that are probably psi-related from those that are probably not. Earlier anthropological theories of magic have been briefly reviewed to illustrate the basic phenomena with which an adequate theory of magic must deal. The processes of metaphorical predication and analogical modeling have been suggested as underlying the development of an integrated complex of magico-religious phenomena. A theoretical perspective on magic, based upon the psi-related principles of magic (as revealed through the correspondences of magic and parapsychological research) and the processes of metaphoric movement and the universal laws of human thought, has been outlined. Further progress in the explanation of magical beliefs and practices will require that anthropologists closely examine the aspects of magical systems which may involve psi. The destruction of traditional social systems with the worldwide advance of industrial society has destroyed many magical systems, but numerous practices survive and require investigation to determine their possible empirical and experiential bases. As an adjunct to traditional ethnographic method, this work should utilize scientific methodology and experimental controls.

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Winkelman's valiant efforts are vitiated by what I will call the *mise en scène*—the desire that magical acts, empirical bases of magical acts, etc., *should* be true. Whatever erudition Winkelman and, prototypically, Mircea Eliade possess tends to become ancillary to that wish. Many anthropologists working with the religious and magical sphere have, Winkelman's claim notwithstanding, accepted magical phenomena "as reported"; what they deny is the causal concatenation the practitioners and their clientele believe to be at the base of these acts. When a pilgrim at Kataragama has his cheeks and tongue penetrated by a metal skewer, when he swings from a scaffold suspended by hooks dug into his flesh, and when he firewalks, without pain or bleeding, the reasons he and other pilgrims give—that he has taken a vow, that he has eschewed meat, fish, and sex, and that Lord Kataragama has blessed him—are good for the anthropologist to think and note, but very bad to accept. If emic statements are to be etically true as well, they must be subjected to the whole line of Popperian strictures. Thus, when I saw a yogi walk on water I would have listed a number of possible causes for the event including my fatigue and excitement, but not his claim that a particular goddess had given him *siddhi* (magical power).

In the volume Winkelman quotes and relies on *in extenso*, I have stated my reasons for excluding parapsychology and *psi stante pede* from anthropological research, especially on religious performance (Bharati 1977). Winkelman's doctrine that magic has an ontological base and that this base is *psi*-related is a prop of the *mise en scène*. Rhine's card-pack and other rather boring, rather trivial performances supposedly generated by a *psi* factor belong to one set of events; magical acts, as Winkelman would no doubt admit, belong to another, since magical acts are intended for an audience, whether or not that intention is admitted by the practitioner. That audience is not the believer in *psi*, but the cultural group that generates and witnesses magical acts. I therefore do not believe that an anthropologist can observe magical acts in the manner in which he observes bifurcate collateral kinship or even ritual performances with no emic magical connotations, unless of course the anthropologist also happens to be part of the society transacting the magical acts and not alienated from it by his training, vocation, etc.

What *are* "magical universals"? And granted that a list could be produced, why make any ontological claims for its members? Winkelman seems to think that cross-cultural occurrences of magical acts must have an ontological base because they occur cross-culturally. This is a non sequitur. Cross-culturally present templates may or may not have an ontological base. The question is not whether or not they do, but whether an ontological base is required at all. I do not like phenomenology, but I think Husserl was right when he said you have to "bracket out" (*ausklammern*) existence from any report of phenomena. Since long before him, the Buddhists have done very well indeed, philosophically and intellectually, without any ontological claims.

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Given the current wide appeal of parapsychology and the public discussion surrounding it, Winkelman's paper provides a welcome opportunity for an exchange of views among anthropologists on this topic.

1. Winkelman argues that "anthropological theories about the techniques broadly labeled magic . . . have been distorted by *lack of awareness and/or acceptance* of the basic assumptions of magical belief" (italics added). If one were to *accept* the basic assumptions of magical belief a priori, little further explanation of the beliefs would be required; they would simply be seen as appropriate statements about the nature of the world. With regard to what he terms "*psi-related*" magic this is, in effect, what Winkelman is asking us to do, at least provisionally. He himself notes that the evidence to demonstrate "whether or not *psi-related* phenomena may provide an empirical basis for such belief" is not now available.

2. Winkelman speaks of "the universal principles of magic, arising out of the associational structures of the mind," and "the application of the laws of association." This is reminiscent of Frazer's theories of magic, which incorporated an associationist psychology that is now outdated.

3. We may ask to what extent the "universal principles of magic" presented by Frazer may not be the product of his comparative method and of the tradition he established for generations of textbook writers. Individual ethnographies do not show us the operation of a full panoply of the "principles of magic" in individual cultures. Indeed, as Honigsmann has reminded us (1976:219), Radin long ago pointed out that the role of magic in "primitive" societies has been exaggerated by anthropologists.

4. Winkelman claims that "a central characteristic of magic practices is the alteration of consciousness." In this context, he appears to make "shamanic power" and "magic" synonymous, which is surely unwarranted. The altered state that signals the shaman's call may be psychotic, drug-induced, or the result of a variety of other factors; is this magic? Or *psi*? It is true that ritualized altered states of consciousness are virtually universal among human societies, but this in and of itself is not evidence for *psi*. Magic, to the contrary, is a highly sober, often purely mechanical procedure.

5. Winkelman suggests "that some magical practices facilitate or produce empirically verifiable effects outside of the currently understood cause-and-effect processes of nature." A good many items of ritual behavior presented in the ethnographic literature as "magical" and "irrational" have turned out to produce "empirically verifiable effects" when the processes and substances used were adequately investigated. The clearest instance concerns pharmacologically active substances, where a distinction between the ritual elements and the active principles is not made by the traditional healer (e.g., the Yoruba priest shown in Raymond Prince's film, *Were Ni*, speaking to the *Rauwolfia* tree while gathering the plant materials; see also Prince 1964). One would need to be thoroughly satisfied that *all* aspects of a given situation had been investigated and fully accounted for before looking "outside the cause-and-effect processes of nature."

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Winkelman is to be commended for his excellent examination of *psi*-related aspects of magic not generally discussed in the anthropological literature. With notable exceptions, anthropology has begged the question of *psi* phenomena throughout its history of analyzing non-Western cultures, in which such phenomena have been amply recorded, generally codified under terms like magico-religious behavior. Recently, both in publications and in symposia, *psi* phenomena have been the focus of numerous anthropological reports (see esp. Long 1977*d*, numerous articles in *Phoenix*, and my own extension courses on

paranormal phenomena at the University of California, Berkeley, Los Angeles, and Irvine).

Winkelman is correct that many anthropologists are fearful of publishing their data on any psi events they might witness or record from their informants in the field. My graduate anthropological training did not prepare me to code the paranormal events that my Amazonian informants continually told me about in the wake of their ayahuasca drug experiences during healing rituals or as an explanation for the misfortunes they suffered because of their fears of bewitchment. While my personal response as an outsider was that my informants were slightly confused as a group and had bizarre and unexplainable experiences, I had no way to accommodate the information I was given or observed into any taxon I carried about in my head. Thus, I may have lost a valuable source of data about urban Amazonian residents' world view or psi experiences. By the same token, experimental parapsychology, with its pretensions to scientific expertise, is no help to the anthropologist facing this existential dilemma in the field. Measurement devoid of a theoretical schema is quite without meaning. Winkelman's call for a reformulation of anthropological theories to include psi deserves to be heeded. While it may be outside the immediate scope of his paper, he might also have pointed to the literature on theoretical physics and subatomic-particle theory. As Capra has argued (Miller 1981), fundamental changes are occurring in our view of the physical universe. The Cartesian world view is a mechanistic one; it envisions a multitude of separate objects working together like a huge machine. There is a strict separation of mind and matter. This observed machine-like universe operates according to causal laws; everything that happens has a definite cause and gives rise to a definite effect. The validity of the observer as separate from the world he is describing is a basic tenet of the scientific method. Capra and others have shown that in the last decade in physics a totally new paradigm has emerged, one which has important implications for anthropology in general and psi phenomena in particular. The main lesson of subatomic physics is that there are no *things*, but only interconnections between things. The physical world is a web of relationships. Further, the scientist is not an observer, but a participant in what he observes. Most of the methodology used in experimental parapsychology is of the Newtonian genre and is mechanistic. New techniques and ways to measure the observer effect are necessary in the social and behavioral sciences, as well as in physics. Certainly, Castaneda's work (1968, 1971, 1972, 1974, 1977) leaves us with this notion (despite the controversy about the truthfulness of the material presented).

Anthropological topics like witchcraft, sorcery, divination, and curing, referred to by Winkelman, have been studied anthropologically with old paradigms. It is only by attending to the shift in the nature of "reality" that we can benefit from our anthropological data bank to make sense out of psi phenomena. My own researches in plant hallucinogens have shown that most of the world's peoples use LSD-like plants for their divinatory properties to permit them access to information not normally accessible to them in ordinary consciousness (Dobkin de Rios 1976). This information may be about future events, but in many cases it is retrocognitive knowledge of past events which is used to make decisions about future actions (1978). From a mechanistic perspective, this kind of data would at best be relegated to a grocery list of belief systems, examined for its symbolic value or else correlated with some other "thing" in culture. Bell's theorem in physics suggests that protons can travel backward in time. Drug-related altered states of consciousness used to divine the future or to give insight into past events may not have to be viewed merely in terms of Amazonian natives' magical beliefs or folklore. Rather, positive expectations held by members of that society

that past and future time is accessible under pharmacological intervention may in fact make such information available to them.

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Anyone who calls for a theoretical reassessment of magic should have consulted previous scholarly discussions of the subject. Although Winkelman cites Tylor and Frazer from the 19th century and Mauss and Malinowski from the early 20th century, he makes no reference whatsoever to any of the numerous anthropological debates over the difficult question of the definition of magic (and its inevitably fruitless attempt to differentiate it from religion). While one might excuse the failure to consider early surveys (Thomas 1904, Leuba 1912, Goldenweiser 1919, Clemen 1921, Evans-Pritchard 1933, and Webster 1948), it seems odd that Winkelman has ignored contemporary theoretical reassessments of magic (e.g., Wax 1963, Clausen 1969, Hammond 1970, Sarga 1974, Rosengren 1976), especially since several of these appeared in *CURRENT ANTHROPOLOGY*. The parochial nature of his knowledge is further suggested by the lack of reference to any non-English language sources. From this one might wrongly infer that no significant theoretical work on either magic or parapsychology had appeared in any language other than English. In parapsychology, for example, some of the most interesting research has been carried out in the Soviet Union (see Ostrander and Schroeder 1970).

Winkelman claims that anthropologists "have rarely accepted magical phenomena as reported," but folklorists have collected such data, including parapsychological materials, for decades. Winkelman does quote folklorist Andrew Lang from the 1890s, but he seems totally unaware of any of the recent folkloristic scholarship. One thinks of the field research of Finnish folklorist Virtanen (1976), Grober-Gluck's (1974) investigation of second sight in northern Germany, and St. Clair's (1972) survey of Irish data, among others (see Sannwald 1965; Harkort 1968; Wimmer 1972, 1975; and other sources cited in Assion 1975). Anthropologists too have studied the intersection of magic and parapsychology, e.g., Stumpfe's (1976) consideration of magically induced death and Brown's (1978) comparison of "foresight" in Mexico and Ghana.

The major difficulty in Winkelman's essay, however, comes not from bibliographical lacunae, but from the very attempt to make a detailed comparison of two such fuzzy, ill-defined analytic constructs as "magic" and "psi." Both are rubric terms whose constituent elements are far from clear. Even so, it is possible to show that the bulk of "psi" phenomena simply do *not* fall into the area of magic.

There seems to be general consensus about the principal alleged manifestations of "psi" (Heywood 1978:17; cf. Murphy 1961). They include telepathy (direct awareness of the mental state of another person), clairvoyance (direct awareness of a physical event), precognition (foreknowledge of the future), and retrocognition (knowledge of the past). Magic, in contrast, involves some kind of instrumental or causal process or procedure by means of which events are produced or controlled. Many writers on magic have specifically drawn attention to the "coercive" nature of magic (Leuba 1912:350; Clausen 1969:141; Hammond 1970:1352; Rosengren 1976:671). Leaving aside the thorny question of the rationality of magic (Evans-Pritchard 1933, Preuss 1938, Settle 1971)—though presumably all magical acts conform to one or more culturally relative logics—it can legitimately be argued that magic in the more rigorous, narrow sense of the analytic term does imply the influencing or manipulating of nature in some

causal way. The point is, then, that whereas "psi" involves *knowledge* of events, magic involves *causing* events. Knowing something is *not* the same thing as making it happen. Prediction is not production. For this reason, it seems unlikely that most of the traditional "psi" phenomena (telepathy, clairvoyance, precognition, and retrocognition) have much to do with pure magic. (Psychokinesis might be an exception, but does Winkelman think that rituals intended to produce rainfall or to ensure a sufficient game or fish supply are truly analogous to the claim of a possessor of "psi" in Western culture to bend metal through mental concentration?)

Parapsychological phenomena have been and should continue to be investigated cross-culturally, but they are more likely to be germane to studies of divination than to magic. The growing interest in parapsychology among scholars is itself part of a discernible cultural pattern in Western culture (see Truzzi 1972), a pattern no doubt related to the surges of popular fascination with reports of poltergeists, UFO sightings, and the like. The continued scientific investigations of the Shroud of Turin, Bigfoot, and the Loch Ness monster demonstrate both that supernaturalism is not dead and that scientists have been unable to resist the appeal of studying such phenomena. It might also be observed that the increased interest in parapsychology has made some conventional academics uneasy, so much so that a prestigious Committee for the Scientific Investigation of Claims of the Paranormal has published *The Skeptical Inquirer* (*The Zetetic*) since 1976.

by JULE EISENBUD

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Winkelman's paper is rich in ideas, but I would have liked to see some attention given to the historical and developmental aspects of the subject. Even though this would necessarily have to remain speculative, it could add to the coherence of what has been presented.

What has to be filled in conjecturally is the period between a presumptive initial phase of man's history in which, from a number of indications, the role of psi was taken for granted in the way things worked (and here parapsychology can contribute to the empirical base of such a presumption) and the late historical phase in which the individual's influence on outer events is seen as limited to sensory-motor contact with other organisms and things. A reasonable supposition is that mana and its equivalents (orenda, wakanda) was an intermediate stage in the exteriorization, or projection outward, of psi-implemented intent in causality (Eisenbud 1967, 1981). (Such a transformation might account in part for the seeming confusion as to exactly what mana actually is.) Psychology, particularly psychoanalysis, can provide a reasonable hypothesis as to why such a projective trend developed, namely, increasingly intolerable guilt about the destructive aspects of psi. This might have followed upon the change from small hunter-gatherer populations to dense agrarian ones. Magic, in this view, would be seen not only as a technology oriented toward augmenting the efficacy and reliability of such psi as may have been irregularly manifest, but also as the means of manipulating mana (as Malinowski had presumed) and the variety of spiritual entities also arising as projections of psi and the (mostly malevolent) human will.

It is a good presumption, thus, that earliest man did not experience psi as something apart from nature, to be commandeered for individual (or particular group) advantage. According to some (Cornford 1957, Kelsen 1943), social structure and nature, as experienced in earliest times, were intimately related and were in fact seen as images of each other. Psi, in such a context, would have been experienced as inseparably a part of both and no more to be withdrawn from its imbeddedness in

the total picture than consciousness itself. According to this view, it is only when the idea of using psi for individual advantage took shape that magic and mana could have eventuated. And this is where Winkelman begins.

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Winkelman is to be commended for his effort to point up the larger cultural and psychological context for psi phenomena. I should like to suggest, however, that a more powerful generalization can be applied than the one he offers. He enumerates altered states of consciousness, goal-oriented visualization strategies, divination procedures, and influences upon indeterminate systems as those aspects of magic that are related to psi. On the basis of the relevant ethnographic literature, fieldwork observation (Goodman 1972; Goodman, Henney, and Pressel 1974), and experimentation (Goodman 1977), I would propose that these four aspects are in fact related in a systematic manner: Goal-oriented visualization strategies are among the many methods for entering the (religious) altered state of consciousness. This is the biological substratum which makes possible the perception of an other-than-ordinary dimension of reality, in which the so-called laws of nature, discovered by Western science for ordinary reality, do not obtain. Especially the limits of time and space are invalid. The absence of these makes divination possible. Switching into the religious altered state also endows the subject with the ability to affect indeterminate systems, as well as a number of determinate ones, in a manner some Western observers have termed magical. The problem with psi research is not only that it has to work with subjects whose access to the religious altered state of consciousness is often fortuitous because they are not privy to the traditions which permit easy and reliable entrance into it, but also that it must deal with data that have been subjected to a double distortion—first transposed from the alternate plane of existence to the ordinary one and then translated into the Western cultural medium. In addition, Western psi researchers are constrained to use for their tests methods developed not for processes originating in the alternate reality, but for those taking place in the ordinary one.

by C. R. HALLPIKE

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Cultural relativists, Durkheimians, and complacent materialists are all likely to be dismayed by Winkelman's paper, which presents a well-documented argument that magic is not simply an illusion, or false reasoning, or a functional device to support the social order, but in part a manifestation of a universal and genuine human ability, psi. The sifting out of those aspects of magic most probably related to psi factors from those of purely social origin seems to have been particularly well done, and the resemblances noted in the functioning of psi and magic should provide fertile hypotheses for testing in the field.

Establishing just how psi has been incorporated into culture seems likely to present greater difficulties, however. Winkelman considers that, apart from psi, "the other primary aspects of magic are the social ritual and the metaphysical or cosmological." This omits cognition, and in my *Foundations of Primitive Thought* (1979:474) I pointed out that preoperatory thought is a favourable basis for the development of magical thought. If cognitive factors are added to those discussed by

Winkelman in the genesis of cultural systems of magic, the processes involved are likely to be especially hard to disentangle.

One suspects that, as Winkelman says, more anthropologists have encountered paranormal phenomena in the field than is apparent from the ethnographic literature. Perhaps they may now be emboldened by his paper to come forward in print with these experiences—in a future symposium in CA?

by ÅKE HULTKRANTZ

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Paranormal data have largely been silently passed over in serious anthropological works, including this journal. This is deplorable, for whether you accept their reality or not they should be discussed and evaluated by students who so often confront seemingly unexplainable events in their fieldwork. It is to Winkelman's great credit that he dares to deal with this kind of phenomenon in his theoretical research.

For my own part, I consider it legitimate to work with the hypothesis that some so-called magical phenomena may be referred to the operations of psi factors. A close follow-up of parapsychological research and incontestable personal experiences, some of them in the field (cf. Hultkrantz 1981), have convinced me of the possibility that we must reckon with dimensions of reality beyond those we usually distinguish. Unfortunately, such a statement is still to most colleagues a statement of belief, when in fact it should be understood as a perception of what Kuhn has called a new paradigm. Certainly, the consequences of such a realization would be most drastic for anthropological theory, and I imagine that the conventional mould of anthropological thinking would inspire a vigorous defence against what one colleague calls "the moral authority of obscurantists and mystics" (Harris 1980:75). With this declaration I do not say that I subscribe promiscuously to the authenticity of all supposed paranormal phenomena. I think that Winkelman shares my view here; it would have been helpful if he had stated his position more clearly.

Unlike Winkelman, however, I do not see any reason to postulate that magic as such originated in psi phenomena. There is, for example, no doubt that many magical beliefs and devices proceed from neurotic obsessional ideas: I have to reach my door before that car is in line with it or my luck will run out; I have to pray with my right thumb over the left one, not the other way round, for then I invoke the devil; and so on. The author points out that emotionally loaded magic may involve psi, and that could certainly be correct (just as emotional religious acts may reveal the action of a psi factor). The characteristics of psi-related magic listed in table 1 can be narrowed even more by saying that it is primarily in connection with altered states of consciousness that the psi factor becomes evident. These states, whether we call them magical or religious, should somehow be able to release psi. Shamanism, divination in certain forms, possession, mysticism, and similar ecstatic or unconscious conditions are thus interpreted to invite psi, to constitute prerequisites for its operation. I do not find that the reference to magic as such makes much sense here. Therefore, I would prefer to see Winkelman's paper as a proposal of investigation of the relation between psi and altered states of consciousness, magical or not.

The author's exposition of magic is very interesting, and his distinctions of the levels of interpretation and their mutual connections are well developed. His strictures on symbolist interpretations of magic are to the point and need to be repeated: these interpretations do not reveal the ontological basis of magic, but demonstrate its socially integrated ritual forms. I miss, however, the European discussion of whether we can separate a particular category of magic (American au-

thors tend to confine themselves to American sources and debates, unfortunately). Finally, the interrelation of magic with mana—a highly doubtful constellation—leads us astray, I think, in our understanding of magic, and it definitely does not point in the direction of psi experiences.

I repeat what I have said before: the great merit of this paper is that it focusses on problems which have long been bypassed by anthropological journals.

by I. C. JARVIE

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Anthropologists have been debating for some time what standing should be accorded the conceptual schemes—better, theories of the world and their associated conceptual schemes—of the peoples they study (at last report, most of mankind). The debate is not peculiar to students of alien societies. Adam Smith and Emile Durkheim, to name only two, introduced theoretical and explanatory ideas that superseded common-sense views on, respectively, the economy and suicide. The study of magic and religion stands like a crucial experiment at the centre of this debate. Both magic and religion postulate causal forces at work in the world of which the orthodox scientific tradition has long been sceptical.

One of the most vigorously pursued lines of this debate in recent years was sparked off by Winch's (1964) philosophical argument to the effect that in religious and magical matters there was a presumption of legitimacy for indigenous theoretical/conceptual schemes. Azande witchcraft, he argued, was a self-contained universe of discourse, and each universe of discourse constitutes a conception, and hence is its own judge, of what is real. That is, there is no privileged or meta-universe of discourse within which other universes of discourse can be appraised as to the extent to which they correctly capture a transcendent "reality." What God's reality was or was not, Winch declared in a provocative formulation, could only be settled within the universe of discourse of god-talk. Where witchcraft was at work and where it was not could only be settled within the framework of witchcraft talk and assumptions, not, as Evans-Pritchard (1937) did, by declaring it illusory as such.

The fundamental problem for Winch's position is what standing it gives to certain expressions—common in universes of discourse with which I am familiar and not, I am sure, absent from Winch's—such as "religion is the opiate of the people" or "religion (or magic) is a delusion and a fraud." Such expressions, which attempt to assess a universe of discourse as such, must belong to *some* universe of discourse, but Winch has yet to give a clear answer to the difficulties they pose for him.

Perhaps it was only a matter of time before Winch's plea for not explaining magic away, i.e., for accepting it at its claimed or face value, would be followed by a plea that magic be taken literally in a nonmagical universe of discourse such as anthropology. Nevertheless, there are features of Winkelman's paper that make one wonder whether CA's illustrious editor is not pulling his colleagues' collective leg in publishing it. Its argument is extremely weak; in summary, it would read, "we should take magical causation at face value because there are some people in our own society who take seriously some phenomena that are in some ways reminiscent of magic (and in some ways not)." This weak argument is in turn premised on resemblances between mana and psi that are tenuous and vague (psi = factors or processes that transcend accepted laws, i.e., mysterious forces). Furthermore, all the phenomena Winkelman refers to (magic and psi) have alternative, unmythical explanations, and most of the people who take it all at face value are on the fringe of the intellectual world.

But if the publication of Winkelman's article is a jape, it is being played with a straight face, CA☆ treatment and all, so I must follow suit. One obvious reminder is in order: regardless of the intellectual standing of the conceptual scheme of magic, there is a place for the sociology and anthropology of it. Science, the intellectual standing of which is not here in question, is studied in this fashion.

With that said, I turn to why it is that magic and religion pose special problems. Winkelman seems to think it is because they postulate mysterious or transcendent factors at work in the world. This is his mistake: orthodox natural science does that all the time. Most new scientific theories are so classed at first. The problem is not metaphysical, but methodological: either the claims of magic and religion are so vague that there is no way to falsify them, and falsifiability is a minimal requirement for serious consideration; or, if they are clearly and sharply enough formulated to be testable, they are so manifestly embedded in local culture and society that there are plentiful intellectual-cum-social mechanisms for turning aside and explaining away failure (Evans-Pritchard 1937; Gellner 1973 and 1974). The issue, then, is neither physical nor metaphysical—the four fundamental forces versus an intruder, psi. Rather, it is methodological: just what is being claimed, and can we test it?

It is a philosophical mistake to dismiss Azande witchcraft with the claim that we *know*, still less because we can *show*, that it is false (here Evans-Pritchard got it wrong). If Evans-Pritchard is right that Azande witchcraft explains misfortune happening to this person now and we ask what the scientific world view has to say about such a problem, the answer is "nothing." Individual events are treated by current science and common sense as matters of "luck" or "chance"—in other words, we do not seriously try to explain singular or individual events. The person killed by the ricocheting bullet or struck down by the rare disease is, we say, "unlucky." Yet we have no (longer if we ever had a) cosmology of luck; to invoke luck amounts to little more than saying "it happened." To the questions "why me?" and "why now?" the answer is "why not?"

The logic and methodology of psi are very similar. Some of it is delusion and fraud, as extensive literature not cited by Winkelman attests (e.g., Randi 1975, Kusche 1975, de Mille 1980). Some of it is deviations from randomness that are expectable because randomness is an abstraction realized only in a notional infinity. None of it is intellectually problematic; it is only methodologically problematic. Azande witches migrating from bodies are too transient a "poltergeist" phenomenon for serious test. Similarly, brain waves bending spoons never seem able to deflect meters set to detect their forces. There will have to be a lot of keys bent by a lot of people before "psychokinesis" becomes a repeatable event warranting additions to the laws of physics rather than yielding, as it does now, to the crafts of the Magic Circle. Isolated events are ignored because suggestion, misperception, and self-deception cannot be tested for. Natural scientists themselves do not proliferate ghostly forces to explain the noise, blips, artifacts, and garbage that turn up in their results. They clean them out. The world is explicable only in its patterns, not in its singularity. Magical cosmology, like psychic forces, is above all a cosmology of individual persons, local regions, particular social statuses or ritual conditions, and unique conjunctions of constellations. Over eons of man's history such superstition failed to produce ideas that passed the empirical test, hence failed to give man power over nature (Gellner 1980, Agassi 1979). When the more general and more modest cosmology we now recognise as the root of modern natural science appeared, it grew and was accepted because it "proved its mettle" in empirical test.

by BARBARA W. LEX

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I have great concern about the standards of scholarship manifest in this paper. Having reviewed carefully the guidelines for articles to be published in *CURRENT ANTHROPOLOGY*, I am moved to state that in my estimation this article does not even begin to approach the standard of "highest scholarly calibre" as I believe it is understood among scientists and anthropologists. It might be best to ignore this work, for dignity is conferred by any thoughtful reply. Yet, to ignore it confers tacit approval. The dilemma is apparent.

The use of unclear referents in phrasing misleads the reader into thinking that various ethnographers believe or accept "paranormal" events as actual occurrences rather than as folk beliefs or explanations (especially see the third paragraph of the paper—in fact, *mutatis mutandis*, my assessments of the entire paper are neatly summed up in the first sentence of the fourth paragraph). In addition, it appears professionally irresponsible to offer works by Castaneda as efforts that directly document "paranormal" events. Another problem stems from oblique references to purported insights to be found in the "parapsychology" literature. The references to Honorton (1977) and Palmer (1978), for example, are used to support the contention that certain vaguely defined and ill-specified "altered states of consciousness" have some sort of effect on "extrasensory awareness." More precisely, the "depth" of the *alteration* is deemed the significantly influential factor. Since none of these terms is operationalized, at best the result is pseudo-scientistic jargon.

My sentiments are by no means intended to discourage scientific speculation. However, in the United States the social sciences, including anthropology, appear to have fallen into political disfavor, and perhaps one factor in that process has been their espousal of positions so relativistic that all criteria of judgment appear to have been suspended. Winkelman's paper does not suggest that all other possibilities be ruled out before invoking "paranormal" explanations. Since the various properties of "altered states of consciousness" are themselves incompletely known, it seems premature for anthropologists to introduce any "psi" factors as additional unknown quantities. To do so invites ridicule. It is not my intention to sanction censorship of scientific debate, but it appears equally irresponsible to imply that one sort of an explanation is just as good as another. In summary, Winkelman's work appears to be an abortive attempt to field-test the Rumpelstiltskin Effect.

by JOSEPH K. LONG

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The author's greatest failing here is that he does not make it clear to the audience of anthropologists that a vast parapsychological literature exists on a host of rigorous experiments over the past 50 and especially the last 20 years. Anthropologists who are familiar with this literature will have little doubt that the etic(?) principles of psi (telepathy, clairvoyance, and psychokinesis) represent established fact; those who are not will view this as a return to supernaturalism. (Emic categories like spirits, apparitions, and spirit possession are generally established phenomena also but, involving complex combinations of several different psi, psychological, and other elements as they do, they are not at all well understood in terms of the specific psi contributions to each.) Although 15%

of American scientists believe psi is established and 50% more believe psi is possible and worthy of study (see Tart 1980 and Wade 1980), anthropologists, judging from my observations, have the lowest rate of belief (physicists probably have the highest). This is to be expected, since anthropologists have extensive training about psychosocial variables of supernatural beliefs and thus are more aware of a range of alternative explanations for the possible causes of belief systems. Therefore they may be more enlightened than others (or less so). Serious scholars can quickly enlighten themselves regarding the massive evidence for psi by starting from the key reviews in Wolman's (1977) summary. Failure to do so may result in beliefs as magical and rationalizing as those of any "primitive" (Long n.d.).

Anthropologists should, on the other hand, be disabused of any illusions that ethnography can establish psi with the same high level of verification as in the psychology lab: parapsychology may lead to parapsychological anthropology, but the latter would never lead psychology into parapsychology. The work of Long (1977b), in a case involving hex death, Singer and Ankenbrandt (1980), regarding psychic surgery, and Wendenoja (1978), studying poltergeist, suggests that in general no amount of observation will suffice to establish a psi element convincingly in ethnographic context.

Winkelman's article might better have been titled "A Theoretical Beginning." Although the discussion of intellectualist and symbolist traditions is to be expected here, it is basically unnecessary. These are neither "explanations" nor "theories": stating that some form of human behavior has social functions (integration of the group) and psychological functions (rationalization) is about as profound as saying that mammals strive to eat. Hence I view this article as a first step toward the understanding of and explanation for social phenomena like magic and ritual. Granted, this is a minuscule part of the understanding of but one small part of religious phenomena: anthropological categories (mana, taboo, animism, animatism, etc.) and those more commonly discussed by some parapsychologists (ghosts, out-of-body experience, etc.) are scarcely mentioned here and deserve equal discussion. But I applaud Winkelman for taking this first step toward integrating parapsychological data with anthropological facts and ideas.

I tend to disagree with the assumption that psi is non-physical. Perhaps it is, but only in the sense that the subatomic particles of nuclear physics are nonphysical (if protons, e.g., turn out to be *merely* energy, then perhaps I will stand corrected). In any case, the *effects* of psi are obviously physical, so thinking in these terms merely confuses the issue. Assuming that there is no physical explanation for psi is similarly confusing. Several explanations have been proposed, most from quantum mechanics. Walker's (1977), which is the most complete, is able to account for all the events called "psi" by the application of established principles of quantum theory to changes known to occur at brain synapses. Granted, this explanation is too complicated for most to understand and has not been "proven," but it does show that psi *can* be accounted for within the frame of present scientific theory. My (1977a) pragmatic, materialistic, ecological approach to psi will at some point come into direct conflict with the nonmaterialistic approaches apparently espoused by Winkelman within the philosophical framework of anthropology. There is nothing about psi per se that conflicts with traditional anthropological theory or its evolutionistic-materialistic orientation.

by LEONARD W. MOSS

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Religionists, doubtless attempting to establish prior claim to telekinesis, tell us that faith can move mountains. Whether

psi exists or not is unimportant; given the cultural conditions noted by the author, some social scientists would be forced to invent it.

Central to the discussion is the element of *belief*. Thomas (1928) enunciated the *social fact*: if men believe a thing is real and act in accordance with that belief, the consequences of their actions are real.

Following the earthquake of August 1962 in Italy, the miraculous liquification of the dried blood of St. Gennaro, completely off schedule, gave reassurance to the residents of Naples that their city would be spared from disaster (Moss 1963). An Italian chemical engineer, when queried about the miracle, responded: "It is not true, but I believe it." This seeming paradox indicates that, for some, truth exists at different levels. There is empirically verifiable truth and, perhaps, truth which must be accepted on the basis of faith. Might this acceptance be shaken if the "dried blood" turned out to be ferric thiocyanate or some other discovery of the North African medieval alchemists?

by RICHARD J. PRESTON

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Winkelman makes an interesting and worthwhile point; in rethinking "magic" we should attempt to account for the findings of experimental parapsychology. He suggests a cognitive-systems method for doing this. The impression I extracted from his overcompact and rather spiritless exposition is that magic is not just the poor man's tranquilizer (the Malinowskian view) or his poetry (the symbolists' view), but also a part of his ontology (the cognitive view) and *should* be a part of ours, for good empirical (?) reasons, (still) poorly understood.

My complaint is that he has not gone beyond the literal-minded experimentalists' controls in incorporating into his scheme the simple, subtle, and universal human fact that people are sometimes confused. "Magic" is surely a domain in which perceptions are chronically confused, inadequately expressed in word form, and wrongly interpreted by the receiver of the words. I find it hard to believe that the cognitive-systems method is adequate to examine these kinds of anomalous experiences. How do people experience, perceive, and report anomalies—not only in "magic," but in the wider arena of anomalies, with sasquatch and UFO's? What we have discovered about these kinds of poorly understood phenomena (Halpin and Ames 1980) should inform our rules of evidence and our interpretations in rethinking "magic."

When Winkelman says "many mystics have common phenomenological experiences, such as the perception of a dazzling white light, but their descriptions and explanations vary," he has missed the point that their *experiences* also vary and are a source of confusion to them and to us. To make this all intelligible, with the epistemological and psychological problems that must be answered, is an awesome task. Winkelman's approach is helpful, but it is not enough.

by LOLA ROMANUCCI-ROSS

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Still among us, as persisting cultural survivals, are cohorts of investigators in the various social sciences who emulate and imitate 19th-century physicists in methodology. The definition of psi is an excellent example. Physicists have long referred to phenomena unexplained by "natural law" at any point in time as "residuals," but when this definitional frame is borrowed by psi-cologists, what do we have to work with using an analogue model? Did we at any point in time or do we now have a body of verifiable facts regarded as accepted law in the

realm of "nonphysical" events? If not, is it justifiable to isolate "residuals" to a quasi-apocryphal text whose contents have created sects and cults of true believers and their obverse, scoffers and deniers?

This is not to say that anthropologists and psychologists should not document and openly discuss what they perceive as "paranormal" events. I would argue, however, that we would be farther along the road to concordance and factuality if we reexamined and redefined our concept of "normal." It is a Blenheim palace of thought constructs to speak and write of the paranormal and even of parapsychology.

Winkelman's well-documented paper is a substantive contribution to an understanding of uses and misuses of concepts in this area. His own current research on cognition may be considered acceptable as *within* established canons of method of research on the "nonphysical." Here, however, he occasionally violates the Singer and Ankenbrandt caveat in failing to make clear *whose* observations are being validated and for what purpose (e.g., in the quotation from Mauss on altered states of consciousness). In addition, Mauss's and Codrington's use of Melanesian notions of mana to build an argument should be regarded only as a first approximation. Those who have done research in the field in Melanesia have learned a great deal since those pristine days of anthropological writing, particularly with regard to what Melanesians consider "the regular course of nature." In my experience, mana is not outside of nature (Romanucci-Ross 1978).

I have asserted elsewhere that in emulating the scientist-technologist we err in adopting his logical structures, which are not always appropriate even for the physical world (see Romanucci-Ross 1980). Objects and events do not exist because we create words or metaphors for them. Rivers may flow, but time does no such thing, although it may serve as a metaphor for the physicist. Similarly, the word "psychokinesis" should not be used in rational discourse as though it were an accepted and proven phenomenon; as Winkelman later states, the phenomenon must be investigated to ascertain whether it does indeed have an empirically verifiable basis.

I would submit that the phenomena under investigation would yield their secrets more readily to the recognition that experimental methods which have worked well in the world of matter are more often than not inappropriate to "paranormal" events. Winkelman appears to be interested in developing a new methodology, and one may hope that he will take into account this most important consideration.

by HANS SEBALD

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Winkelman not only tries to compare magic and parapsychological experiences, but labors to make a case for the existence of psi in both phenomena. In the pursuit of this goal, he marshals more speculation than fact. Accepting such definitional elements of psi as "personal factors or processes in nature which transcend accepted laws . . . and which are nonphysical" (exemplified by ESP and PK), he seems to find abundant evidence of psi in both magical practices and parapsychological experiences. While I agree that similar, if not identical, mental mechanisms operate in the two genres, I disagree with defining them as psi processes. Winkelman's argument rests on a gigantic and shaky assumption.

He lists a number of dimensions that magic and parapsychology have in common and that presumably reflect psi. To say that "a central characteristic of magical practice [and presumably also of parapsychological experiences] is the alteration of consciousness" is so general that it cannot be proven incorrect. A glass of wine, a joint, or exuberant sex can also alter consciousness. That visualization is a better means than concen-

tration in ESP performances is a relative statement, since concentration could achieve such insignificant results that even the "significantly better" results by visualization would still linger in the insignificant realm. That a positive and believing attitude can operate as self-fulfilling prophecy is commonplace and, of course, can be applied to the so-called psi experimenter as well as to the psi subject—whether in the context of magic or in the parapsychology laboratory. The results of faith can be explained without recourse to the often obscure and implausible psi phenomena. Such results frequently assume psychosomatic forms. Winkelman is alert enough to know this, and he tries to guard himself against criticism by referring (n. 9) to "psychosomatic" as a sort of vestige of the Cartesian dualism of "mind" and "body." His remark falls short of discrediting the concept, but reflects a weak argument. In any case, no informed academic adheres to such dualism; the unity of brain functions and somatic effects is clear enough.

If, as the author seems to agree with the literature, *faith* is conducive, if not prerequisite, to a positive psi outcome, then psi results have plenty of company among many forms of self-fulfilling prophecy and selective perception. For example, my study of the Franconian witchcraft tradition allowed, without exception, explanation of "magical" (or presumably psi) incidents through perfectly normal physical or psychological processes. Winkelman is not clear enough about the distinction between faith and psychosomatic effects, on one side, and psi phenomena, on the other. Nonetheless, his discussion tries to be fair and includes references to opinions and data that allow suspicion of psi experiences as being camouflaged forms of faith healing, faith diseases (negative forms of beliefs), or, in general, psychosomatic processes triggered by certain brain circuitry.

Throughout the paper, however, the author uncritically accepts the notion that psi in fact exists. This reflects the facile acceptance of the paranormal and the occult that took an unprecedented upswing during the counterculture era and seems to have come into full bloom in the New Age romanticism of the 1980s. I see this credulity as an attempt to find solutions and solace outside of traditional avenues and outside of the type of struggle carried on by secular humanism to come to grips with frustrating, vexing, and agonizing conditions of human existence. It illustrates the current popularity of replacing empiricism with spiritualism, scientific thinking with wishful rationalizations, realistic measurements with mentalistic verbiage. While such intuitive, imaginative, and colorful exercises have their romantic value, they should not be confused with science. Lest I be labeled an academic Luddite, I should like to point out that history is on my side: While science, laboriously and often lusterlessly, has added building block after building block to create an appreciably reliable body of knowledge, the study or practice of psi, in one form or another, has continued throughout history without accumulating substantial proof of its validity and without integrating a body of reliable knowledge.

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Winkelman's article is worthy of close attention for a number of reasons. First, the author does an outstanding job of weaving together experimental results with ethnographic research. Second, his work is on an anthropological frontier that is rich with exciting possibilities. Third, even if one does not agree with his argument or conclusions it must be admitted that he has raised a significant issue. The "psychic" or "psi" has for too long been a "tapu" topic in anthropology. It cannot be said that Winkelman has in fact proven that there is a genuine

"psi" element to magic, simply because no single article can do that, but if his work accomplishes nothing more than to force anthropologists to weigh the issue in terms of fact rather than mere prejudice it will have made a significant contribution to our science.

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[The following comment is of a length not normally accepted for CURRENT ANTHROPOLOGY, but it is in itself a review article with substantial data that open issues that should be debated. The acceptance of a comment of this length is to be considered extremely exceptional and does not indicate a departure from policy in this matter.—EDITOR.]

Winkelman clearly states in n. 3 the problem that vitiates his paper's methodology and conclusions. Annotating the statement that "the relationship between parapsychological research and magical practices is not straightforward and unproblematic," he says that "there are profound methodological and epistemological differences between parapsychology and magic," one of which is that "the paranormal phenomena of parapsychology are generally observable only in statistical deviations from normal expectation, not in the macroevents or outstanding individual performances characteristic of magic." One must wonder why the paper was written, since its main purpose is to compare two apparently noncomparable phenomena. Winkelman's thesis is that the phenomenological existence of magic can be demonstrated by relating it to the (supposedly proven) phenomenological existence of parapsychological functions. The trouble with this thesis is that both the parapsychological and the anthropological literature cited is less than reliable.

Unreliability of several types characterizes the parapsychological material. First, one encounters the problem of just *what* is being cited: the primary article, in which an experiment and its results are described by the experimenter him/herself, or a review of literature or other secondary article in which experiment and results are briefly summarized, often by a writer unconnected with the original project. Both types are valuable, but their evidential value is different. Sometimes the distinction is made here, but of 36 articles cited without such qualification—as though they contained primary data—at least 11 are reviews or some other kind of secondary article.¹ Some of these unlabeled citations appear in contexts that may be misleading because different types of articles are mixed together without identification. In other examples the phrasing suggests a report on original research when the article is actually a review. This method of citation requires constant reference to the list of "References Cited" and often to the actual sources when the information supplied in this list is not sufficient.

At times, the summary of an article's contents is simply inaccurate, as when Winkelman writes: "Ullman (1977) points out that as people near psychotic breaks or crises they manifest much stronger psi events in the course of therapy." Reading Ullman's article, one finds that this point is at best debatable—we will deal with this further below—and also that the article is actually a review, so that the evidence is not immediately available.

This kind of inaccuracy raises more general issues about parapsychological research such as are discussed by Hansel (1980) in terms of Rhine and Pratt's distinction (1957:140; cf. Hansel 1980:24–25 and Rao 1979:163) between "exploratory" and "conclusive" experiments. Most experimental work in parapsychology apparently fails to meet Rhine and Pratt's criteria for conclusiveness. As Hansel (p. 25) summarizes them, these are (1) sound measurement, (2) satisfactory experimental safeguards against normal sensory communication, (3) care in recording (here they remark that the responsibility of recording data should be shared by two persons in such a way that no error made by either can go undetected), and (4) precautions against deception on the part of the experimenters themselves. On this basis, Hansel concludes (p. 314): "After 100 years of research, not a single individual has been found who can demonstrate ESP to the satisfaction of independent investigators." That this is not simply willful skepticism (cf. Rao 1979:159) is clear from the following statements by Murphy (1971:6), a leading researcher in parapsychology:

One factor quite evident here when you look at the phenomena from a psychometric point of view, a point of view based on familiarity with psychological tests of all sorts, is the fact that most of the scoring levels are only slightly above chance expectation. Consequently, only a very small fraction of the variance which appears in the above-chance scores is attributable to ESP; in other words, *ninety-nine per cent of the observed variance is due to factors other than ESP.*

In consequence, both Murphy and Hansel point out that "lawful interrelations" within the body of fact in parapsychology" (Hansel 1980:312; cf. Murphy 1971:8) are for practical purposes nonexistent: "To date, not a single lawful interrelation appears to have been established. . . . each of the reported investigations yields a result that has little relationship to any of the others" (Hansel 1980:312).

To arrive at this conclusion, Hansel (pp. 25–28) examines a number of experiments usually cited by parapsychologists as conclusive.² Using Rhine and Pratt's criteria, Hansel shows that *in no case* is the research design sufficiently rigorous to exclude trickery, collusion, overinterpretation, and the like. Therefore, he argues, the body of parapsychological research must be treated as, in Rhine's terms, exploratory rather than conclusive (pp. 310–11)—and, in fact, he repeatedly notes that as testing procedures for psi have become more rigorous, the high scores reported in the '20s and '30s have disappeared (Hansel 1980:166, 193, 199, 237, 311).

Other criticisms of parapsychological research design—e.g., by Price (cited in Hansel 1980:149–51, 308–9), Murphy (1971), Hansel (e.g., 1969), and others—are easily found, and fraudulent results in scientific research have recently been reported with increasing frequency (Broad 1981:137). Two of the works Winkelman cites (Rhine et al. 1940, Targ and Puthoff 1977) are among those criticized sharply by Hansel (1980: 119–21, 291–96), and using Hansel's criteria it is possible to raise serious questions about the evidential value of many others. Since this question of evidence is central to the evaluation of Winkelman's argument, we will explore some of Hansel's criticisms in depth.

Winkelman cites Rhine et al. (1940) to support the idea that "enthusiasm, motivation, interest, and positive motivation [*sic*] on the part of the experimenter" are important for positive results in parapsychological experimentation. Hansel shows (pp. 106–7) that at least two early experimental series—

¹ The review or otherwise secondary articles we found cited by Winkelman without sufficient identification are Osiris and Bokert (1971), Honorton (1977), Stanford (1977), Kennedy (1979), Morris (1980), Puthoff, Targ, and May (1980), Kennedy (1979), Stanford (1977), Rubin and Honorton (1972), Ullman (1977), and Stanford (1974). Further, Goodman (1977) does not contain the evidential material one would expect in an archaeological site report.

² Hansel (1980: 25–27) cites four authoritative lists, published in Rhine and Pratt (1957), showing four conclusive experiments, Pratt et al. (1940), showing six conclusive experiments, Soal and Bateman (1954), showing five experiments, three mentioned without criticism, and McConnell and Schmeidler (1958), showing five conclusive experiments. Of these, the Pearce-Pratt, Pearce-Woodruff, and Soal-Goldney series were unanimously accepted; Hansel gives a chapter to each.

Turner-Ownbey and Pratt-Pearce—included in the 1940 work and also on the list of “conclusive” experiments are by no means above question. In both cases, Hansel demonstrates, the description of the experiment was gradually modified so that its design seems more rigorous. Turner-Ownbey was redesignated in Rhine et al. (1940) as part of a previously unheard-of “Rhine-Ownbey” series, and the irregular handling of part of the data—the part with results above chance—which was mentioned in Rhine’s (1934) *Extra-Sensory Perception* is not mentioned in the 1940 work. It is clear from Hansel’s account (p. 95) that the data in question could have been simply invented by Miss Ownbey and that Rhine himself must have known of this possibility. Hansel also shows that the results of the Pratt-Pearce series, almost universally accepted as “conclusive” (pp. 25–27), could easily have been falsified by Pearce’s simply watching unobserved from one of several possible vantage points while Pratt, the sender, wrote out the order of the cards that Pearce had attempted to guess (pp. 111–23). Also, the description of the experiment was changed in the direction of greater apparent rigor as it was reported in Rhine et al. (1940) and others of Rhine’s works (pp. 119–21). Whether or not these possibilities for deception were actually realized, the value of the experiments and of Rhine’s 1940 book is certainly questionable.

Targ and Puthoff’s *Mind-Reach* (1977) is the subject of similar criticism. On the basis of a letter from D. Marks and R. Kammann, published in *Nature*, Hansel notes (p. 293) that the judges who were to match the targets with the subject’s reports *were actually given a list of the targets in “correct” order, that is, the order in which the targets were visited by the sender*; that enough cues were given the subject that Marks was able to match up the five targets correctly without ever visiting any of them, “solely on the basis of cues contained in the transcripts”; and that an attempted replication by Marks and Kammann was unsuccessful. Again, until (or unless) these points are somehow explained away, it seems impossible to accept the book as serious evidence for psi ability.

Both Murphy (1971:3–4) and Hansel (1980:313) note that replication of experiments, in most fields of science the normal way of dealing with criticism and establishing a factual data base, does not work in ESP experiments. Hansel mentions repeatedly (1980:99–100, 139, 190, 200 [two examples], 233, 248, 252, 293) that an attempt to replicate a specific parapsychological experiment was made but did not succeed. This is even true of Rhine’s work (Hansel 1980:99–100). Hansel characterizes the failure to replicate as “typical” (1980:251); Murphy (1971:8) observes that the “laws” of ESP “remain benign hunches from about the time of the French and British workers of the 1880’s.” Writing in *Science*, Wade (1973:142) concurs: “Parapsychologists are still unable to satisfy the demand for a repeatable experiment.” Murphy in fact comments:

Those of you who know the field of experimental parapsychology know how very far we are from a coherent system which is ready to replace the Newtonian and the nineteenth-century types of physics. . . . it will take both honest experimental replication up to the hilt and with it an ordered system of ideas before the new science has any chance of establishing itself.

Winkelman cites attempted replications for only two original-research reports: Wells and Klein (1972) for Watkins and Watkins (1971) and Levi (1979) for Morris, Nanko, and Phillips (1979).³ Wells and Klein themselves comment (p. 147)

³ The following passage (Morris, Nanko, and Phillips 1979:147–48, emphasis added) illustrates the great distance between anthropological description of behavior and parapsychological investigation: “The apparatus consists of a four-module multi-purpose testing system designed for a variety of studies. . . . It maintains an internally-generated source of random binary decisions by amplifying Zener diode noise with a two transistor amplifier, then converting the amplified noise to logic levels with an LM 339 comparator. The resulting logic signal is then divided by two to insure

that their “results . . . are obviously less significant than those obtained by [Watkins and Watkins] with the same procedure.” They also note (p. 149) the possibility of “information leakage” because of the research design, although they deny that such leakage actually occurred. Levi does indeed claim that her results “represent an independent replication and extension of the Morris et al. study” but then notes, “Before and during the experiment I had no explicit expectations regarding actual scoring; *I became aware of the Morris et al. study only after [my] experiment was completed*” (pp. 276–77, emphasis added). In a footnote she adds that the reader “should . . . keep . . . in mind while reading the experimental procedures” that her “choice of experimental design and variables was determined by the social-psychological rather than the psi aspect of the study.” And in fact, although the differences between the scores of her goal-oriented and process-oriented groups differed significantly from each other, neither differed significantly from her control group’s scores (p. 282). The subjects’ trial-by-trial predictions of their own hits and misses “were correct about as often as would be expected on the basis of chance” (p. 282). That is, apparently psi ability was not demonstrated. Since the task of Levi’s subjects (getting a Schmidt Random-Number Generator to produce numbers larger than 16) was different from that of Morris, Nanko, and Phillips’s subjects (attempting to influence a light display determined by a Random-Number Generator) (Levi 1979:277; Morris, Nanko, and Phillips 1979:147–48), presumably the design of the two experiments was different enough to rule out the possibility that Levi’s is a replication of the earlier one.

At least two other experiments that Winkelman cites have had attempted replications—Targ and Puthoff (1977) by Marks and Kammann (Hansel 1980:293) and Braud (1975) by Stanford and Mayer (Braud 1975:144)—and at least two others are replications of other papers. Tedder and Monty’s (1981) article, which was not available to us, is identified in its subtitle as “a conceptual replication of influence on a biological system,” and Stanford (1971) has the subtitle “A Replicative Study” (not included in Winkelman’s citation). Braud (1980) characterizes his paper as an attempt to explore a “prediction from Rex Stanford’s conformance behavior theory” (p. 297) and then lists six other experiments based on the same theory, one of which represents an unsuccessful attempt at replication of one of the others.⁴

that equal time is spent in the high and low states. This random logic signal . . . can be sampled and clocked into a shift register whenever a new random decision is desired. A counter-decoder sequencing circuit interfaces this information with a display to the subject and a counter which tallies the number of trials (decisions) and the number of hits (trials in which the decision matched a preselected outcome registered on a special console). In the present studies, the display to the subject consisted of a ring of 16 lights The binary random decisions were employed to advance the illuminated light one step clockwise or counterclockwise, thus producing a “random walk” back and forth on the circle. *The hit counter had been preset to count only clockwise steps as hits. . . . The subject was . . . told that the task was to bias the lights for each run of 16 trials in either the clockwise or counterclockwise direction, depending on the instructions given the subject in a concealed target envelope. . . . The random event generator produced decisions in accordance with the subject’s instructions (hits) 51.86 per cent of the time ($Z = 2.38, p < .02$, two-tailed).* We have not attempted an analysis of the statistics involved in parapsychological research, for two reasons: first, we are not competent to do so; and second, as we have pointed out, Murphy is on record as saying that 99% of the observed variance is due to factors other than ESP.

⁴ Braud (1980:300) refers to a report by McCarthy, Keane, and Tremmel (1979:83–84) that an unpublished experiment by Charles Fox with mung beans achieved “highly significant” results but that their own attempt to replicate it was unsuccessful. Braud considers his Experiments 15, 16, and 17 “conceptual replications”—i.e., rep-

Another deficiency in parapsychological research design, which again Hansel (1980) repeatedly mentions (e.g., 92–97, 102–3, 116–17), is the possibility, noted above, of “information leakage” from experimenter to subject. In addition to the works already mentioned, other articles cited by Winkelman involve this problem. The results of Dean and Nash’s (1967) plethysmograph experiment look impressive until one looks closely at the design (room plan, p. 2; description of method, pp. 2–6) and realizes that the subject might have been responding to subliminal vocalizing and perhaps other subliminal cues heard through a hole in the wall. The experimenters themselves admit this possibility (p. 11), maintaining that it does not invalidate their experiment but noting that they intend to avoid the possibility in the future. Grad’s experiments with healing of wounded mice and growth of traumatized plants (Grad, Cadoret, and Paul 1961; Grad 1963, 1964), all involving the allegedly supernormal healing powers of Oskar Estebany, become less impressive when one realizes that Estebany was employed to tend both the mice and the plants during the time when the experiments were being conducted (Grad 1963:129; 1964:492; cf. Grad, Cadoret, and Paul 1961:18–19), so that the healer had constant contact with the research subjects. Grad maintains (Grad, Cadoret, and Paul 1961:19) that “interference [by the healer] was most unlikely,” but, again, these experiments are not conclusive. Since the subjects of these experiments are close to our own professional interests, we telephoned Grad on July 27, 1981, to ask whether his experiments had been replicated. He told us that he is aware of several attempts at replication which he has been informed were successful. One has been privately published (MacDonald, Hickman, and Dakin 1976); the others have not been published at all. He himself feels that his work is conclusive. When asked whether his research design—specifically, the presence of Oskar Estebany, the healer, as employee helping to care for the mice and plants that were the research subjects—had been criticized, he replied that so far as he knew it had not. His earlier plant experiments were “only semi-blind,” but the last one, published in 1967—to which Winkelman does not refer and which we have not seen ourselves—was, he thinks, arranged in far too complicated a fashion to be tampered with even though Estebany might have had the opportunity. In any case, he characterized the reception of his work as “mainly silence”; he is aware that most other scientists—he is by training an endocrinologist—seem not to be interested in following up the research leads his work provides.⁵ Forwald’s (1954) PK experiments on the sideways bouncing of dice, which Winkelman also cites, are still less conclusive because *Forwald acted throughout as his own subject* (Forwald 1954:224). This research design was criticized by J. F. Nicol and by E. Girden (Hansel 1980:195–96), the latter being quoted by Hansel as saying that “Forwald’s 1954 work began some nine years after publication of *Extrasensory Perception after Sixty Years* [Rhine 1940], and yet by the standards of that book, all his data would be unacceptable.”

Demonstrating the phenomenological existence of either ESP or magic—even by fitting one or both into present-day physics or metaphysics, and still more by discovering that fundamental changes in either or both of these systems are necessary because of the nature of psi or mana or both—would require

lications of a *concept* (p. 313) rather than of an experiment. Braud’s Experiment 17 “was inspired by” (p. 310) an experiment reported in Rush (1979), but different mechanical devices were used by the two experimenters; in neither case were the results more than marginally significant (Braud 1980:311; Rush 1979:89). In none of these reports is the physical research design described in enough detail to be evaluated.

⁵ Grad says that he will gladly give any interested researcher the benefit of his experience with such experiments. He may be reached at the Department of Psychiatry, McGill University.

a change in the basic paradigm of science. Kuhn (1970) has shown that such changes result partly from fashion—a suggestion that is borne out by the current popularity of belief in psi in the absence of proof—and that they are in a real sense conversions (see Kuhn 1970:185–87, 213–14). At least equally, however, such changes result from an accumulation of anomalies, apparent violations of paradigm-induced expectations (Kuhn 1970:114–15). As Kuhn points out (1970:chaps. 6, 7; cf. Hansel 1980:310–11), scientific paradigms change to accommodate information or to solve problems that cannot be handled by the old model; but in the field of parapsychology, despite the industry and imagination of many researchers—and their readiness to suggest possible new paradigms for general adoption (e.g., Dobkin de Rios 1975; Long 1977:233–42)—there seems to be a dearth of well-documented, factual material that, *in this special way*, does not fit.

The part of Winkelman’s argument that depends on anthropological evidence is just as little proved. Before looking at material that allegedly supports the phenomenological reality of magic, we repeat that magic is quite unrelated to parapsychology; in fact, we trust it is clear from the foregoing how little parapsychology—with its experimental methods, statistically expressed conclusions, and predictive intent—has to do with cultural anthropology, the normal method of which is participant observation (Wax 1971:3–14; Powdermaker 1966:10–12; Devereux 1967:3–17; Haring 1947).

Winkelman’s use of anthropological literature for “proof” of the reality of magic is as distorted as his citations of parapsychological literature. Although Winkelman cites our paper (Singer and Ankenbrandt 1980), he does not seem to have grasped its point—which was not simply that “anthropologists have failed to make clear whether [their] reports are [i.e., represent?] their informants’ own observations or beliefs, their own observations, scientifically established occurrences, or ad hoc rationalizations,” but that, *because of this kind of inaccurate reporting*, the bulk of anthropological writing about allegedly paranormal occurrences is no more than anecdote. Consequently, we maintain, it is necessary to film allegedly paranormal events, test their results, utilize trained observers as well as magicians, and make use of any other effective way of securing data that will have evidential value.

Winkelman does not comment on these recommendations. However, he seems to hold out a promise of anthropological evidence when he says that “anthropologists have recently begun to document paranormal points directly.” The examples he gives which we have been able to obtain do not support his assertion. For instance, he says that “Castaneda’s . . . reports of paranormal events have strained the credulity of many (e.g., de Mille 1976, 1980),” implying that these books are solid ethnography but so marvellous, unusual, etc., as to be difficult to believe. This is not at all the burden of de Mille’s two carefully documented works. Rather, de Mille throughout questions Castaneda’s scientific credibility and the factual basis of his writings. De Mille’s contention is that the Don Juan books were put together from materials that Castaneda got either from books or from his friends. In our experience, also, verbal parallels—for instance, to the writings of Alan Watts and Fritz Perls—are numerous and easily identifiable (Ankenbrandt 1980:35–42). Thus, Winkelman’s implication that Castaneda’s writings somehow support the phenomenological reality of either psi or magic seems at best factitious. Of the other materials cited in this paragraph, we have succeeded in examining only Sharon (1978). This work makes the claim—normal in descriptions of shamanism (cf. Howells 1962[1948]:127)—that the wizard Eduardo has telepathic and clairvoyant powers and that Sharon acquired similar powers while serving as his apprentice (Sharon 1978:30, 47, 113). Both claims may be true, but if they are to function as evidence of anything more data, preferably supplied by an observer uninvolved in the

shamanistic process, are necessary. We would hope that Sharon would participate in research designed to show whether he and Eduardo actually have such powers. That "other anthropologists may also have observed paranormal events in the field but remained silent either because of the fear of censure . . . or . . . lack of an appropriate theoretical framework" seems pure conjecture; at any rate, we know of no cases to support it.

As with the parapsychological material, Winkelman also refers to secondary works—e.g., Bourguignon (1979)—rather than to the primary articles on which the point in question (cf. Bourguignon 1979:67–68) is based and to works with poor research design—e.g., Rose's work (1955; Rose and Rose 1951) with Australian Aborigines, which is discussed below. He does not mention the part of our 1980 article in which some of the difficulties of doing research with a folk healer are explored. As a result of this experience and of the controversy arising out of it (Singer and Ankenbrandt 1981), we are sharply aware of the difficulties of attempting to understand—or even to observe accurately—what exactly, in phenomenological terms, a folk healer or shaman is doing. Nevertheless, we maintain that not only careful observation, but, ideally, filming and sound recording are necessary if the result is to have any evidential value. Perhaps most evidential is the healer's ability to replicate his/her own feats under conditions that would make sleight-of-hand impossible. The Filipino psychic surgeon Juan Blance, who worked with human subjects in public demonstration sessions in Michigan during 1979 (Singer and Ankenbrandt 1980, 1981), apparently has this ability; even so, further testing and better control of test conditions will be necessary for complete certainty. The fact that a person is an effective healer or seems to have paranormal abilities while working under uncontrolled conditions *does not constitute proof*.

Again, of this issue Winkelman seems unaware. Much of his material, for instance, Lang (1894) and de Vesme (1931), is anecdotal, as he notes. In neither of Barnouw's articles (1942, 1946) does he indicate belief in the phenomenological reality of mediumistic or shamanistic events; in both he is primarily concerned with the cultural shaping of such phenomena (1942:141–42, 146; cf. Singer and Ankenbrandt 1981), and in both his mode of analysis is psychological rather than parapsychological (1942:160–67; 1946:18–21). His earlier article ends with the point that "over and above pathology and professional gain, there are many little-understood motives at work both in mediums and in those whom they serve" (1942:167), so that further research is essential. Elkin is at least as skeptical. Far from having "studied Australian Aborigines' reported paranormal abilities and suggested that they were related to Rhine's experimental parapsychology," Elkin nowhere mentions Rhine; he three times refers to G. N. M. Tyrrell's work (Elkin 1978[1945]: 42, 44, 46, and notes 4, 7, 12), but only for confirmation of points about psychical research that *might* shed light on the Aborigines' reports. Throughout, Elkin's approach is skeptical, and his explanations tend to be rationalistic: for instance, in discussing an unusually circumstantial report of an Aboriginal "clever man" 's alleged supernatural powers, he notes that "the explanation must be sought in group suggestion of a powerful nature" (1978[1945]:54). Like most anthropologists, he did not observe in person the allegedly paranormal material he is reporting. His principal interest is in the character and training of the "men of high degree" in the context of local systems of belief and behavior.

The citation with which Winkelman's paragraph ends adduces a work by Long that we have examined at length (Singer and Ankenbrandt 1981), arguing that many scholars—Long among them—and others concerned with the investigation of allegedly paranormal phenomena are so committed to either a positive or a negative view that they fail to consider evidence

on its merits. The evidence on which Long's positive conclusions rest is not at all conclusive. In 15 anthropological accounts cited by Long, evidence for psi phenomena turned out to be either nonexistent or based solely on the claims of an informant. Four others are possibly evidential but depend wholly on the unsupported word of the anthropologist. This kind of evidence is at best anecdotal—an invitation to research, not a substitute for it.

Four of the works that Winkelman cites in this connection (Rose 1955, 1956; Rose and Rose 1951; McElroy 1955) report parapsychological research with Australian Aborigines. The Roses' two articles (Rose and Rose 1951; Rose 1955) describe testing of subjects under somewhat rough-and-ready conditions for General ESP and PK. The PK tests produced no significant results (1951:122, 129–30; 1955:97–98); the GESP tests revealed one high-scorer, an old woman named Lizzie Williams (1955:127–28; Rose 1955:94–95). The Roses themselves note that their results are not conclusive: they were working under field conditions, with a group of onlookers, and with subjects whose hyperacute hearing is notable (Rose and Rose 1951:125); they paid their subjects in cigarettes and tobacco, not merely for participation but for high scores (1951:124), and considered that older subjects were probably also motivated by cultural pride (1951:124). (As Hansel [1980:309] notes, when there is "a possible monetary or prestige motive for trickery" research design needs to be especially good.) A later series of tests produced "independently significant" scores from 6 of the 12 persons tested, though again Lizzie Williams's results were outstanding (1955:94). In this later report, Rose notes that conditions "would not meet the stringent requirements of contemporary laboratory testing" (1955:93), though he feels that they were "adequate." In his 1956 book, he maintains that ESP was "conclusively demonstrated" (p. 211), but since the research design remained unchanged (1955:92) we cannot agree. McElroy, whose reports Winkelman also cites, likewise notes the difficulties of such work under field conditions (1955:118, 121, 124), the inconclusiveness of the PK test (p. 123), and the discovery that few subjects' scores were impressive (p. 118). Again, given the research design,⁶ his results are exploratory rather than conclusive. Also, like McElroy (1955:119), we would question whether this kind of formal testing of Aborigines is not somewhat "artificial." It seems more valuable to test tribal people—if at all possible—on their own cultural claims to paranormal abilities than to force them into tests of what Western experimenters believe human beings *ought* to be able to do.

In this connection, Winkelman quotes the familiar claim that "the presence of nonbelievers renders magical activities null and void." In our experience it is not true that traditional healers claiming paranormal powers insist on excluding unbelievers. Juan Blance performed seemingly paranormal feats before several groups including a professional magician and composed largely of physicians, whose skepticism and sometimes frank unbelief were frequently obvious (Singer 1979:20; Singer and Ankenbrandt 1980; Ankenbrandt 1979). Later, he admitted that he "can still do the work" under such conditions. Others—Jamsie Naidoo of Guyana (Singer 1973), Chief K. O. K. Onyioha of Nkpura, Nigeria (Singer 1978), Swami Sach-

⁶ Finding card-guessing experiments unsuccessful and time-consuming, McElroy switched to using five tobacco tins, one of which contained a piece of stick tobacco and two of the other four of which contained "an equivalent-sized piece of unscented soap" (p. 121) because "the piece of tobacco rattled at times in laying out tins before the subject." McElroy himself notes Aborigines' "acuity of smell" and cites a study of it (p. 121); it seems possible that either smell or hearing might subliminally or consciously reveal such a difference.

chidanana of Mysore, India (personal communication, 1981), Siddha Baba of Saugor, Madhya Pradesh, India (Singer 1977), "Simon Luke," an Ojibwa tent-shaker and healer in western Ontario (Singer and Ankenbrandt 1981), and even Sai Baba (Sandweiss 1975)—perform before believers and unbelievers alike, and all except Sai Baba have assured us of their willingness to perform also before movie or videotape cameras. We are planning to bring Blance back to Detroit for more evidential testing and to film the various feats of Swami Sachidananda when we have sufficient funds to cover air fare, camera rental, film, and processing. Even though Rhine and others (Wade 1973:140) have doubted the value of studying "the more esoteric aspects of the occult" such as materialization, the shamans are nevertheless there, willing to demonstrate to an unbelieving Westerner the reality of the powers they claim. The numbers of people who seek help from such healers—Juan Blance says that he alone treats from 50 to 70 patients *per day* and Merz (1979:18) speaks of "hundreds of thousands" from the United States, Europe, Canada, Australia, and Japan during the last decades—suggest that it is a moral imperative for scientists to determine the truth or falsity of these claims. But, again, we emphasize that formal testing and especially the use of film, videotape, and sound recording as well as magicians as observers are necessary if we are to understand the processes of folk healing and other traditional behavior involving allegedly paranormal phenomena. This kind of testing and recording will not replace traditional participant observation of the healer or other specialist; rather, the two methods complement each other.

A more pernicious theme of Winkelman's paper is his suggestion that fraud and trickery on the part of the traditional specialist encourage positive expectation in the onlookers *and thereby facilitate psi phenomena*. This idea is difficult to reconcile with the materials he cites to support it. Rose (1956) tends to refer much or all of the Aborigines' claims that they have paranormal powers either to their habitual exaggeration and myth making (p. 204) or to mass hypnosis (pp. 155–66). Lévi-Strauss (1963a) seems to us to suggest that the processes he describes are interpersonal rather than paranormal—the same interpersonal processes investigated at length by Frank (1963), Kiev (1972), Torrey (1972), Singer (1976), and other medical anthropologists. The suggestion that paranormal or psi powers may also be involved may well be true, but it is not as yet substantiated. Reichbart's thesis that trickery is psi-conducive is far less definite than Winkelman suggests; Reichbart is concerned more with the shaman's attitudes towards the powers he/she claims than with the phenomenological reality of those powers (Reichbart 1978: 157–61). What Reichbart substantiates is that shamans are aware of the importance of belief in their audiences, that the participants tend to believe that the shaman has supernormal powers, and so on, not that paranormal phenomena ever actually occur in shamanism (cf. his summary [p. 163] and n. 7). Thus, the main thrust of Reichbart's article is very close to Malinowski's theory that traditional people tend to utilize magic in situations involving uncertainty so as to increase their self-confidence (Malinowski 1954 [1925]:28–32). Other alternative hypotheses are also possible, for instance, one summarized by Wallace, citing Omar K. Moore (1966:172):

Moore points out that in the situation of the northern Indian hunter . . . the strategy with the least likelihood of repeated failures (ending in the catastrophe of starvation) is to randomize his choices [of hunting territory] each year. . . . For such purposes, reading the irregular pattern of cracks and charred spots on the burned scapula of a caribou is as good a technique as any.

But of such alternative possibilities Winkelman seems unaware.

In summary, we say that the case for psi based on the anthropological literature—like the case based on parapsychology

logical research—remains unproven. The field is inviting, crowded with shamans, swamis, and psychic surgeons who profess willingness not just to talk about, but to demonstrate paranormal abilities. We trust that in a few years' time the evidence to prove or disprove their claims will be on the record.

On the basis of the above, we find Winkelman's treatment of theory inadequate. As he himself says, "The concepts of mana and psi are far from being highly congruent." In fact, the parallels Winkelman notes between them seem forced, since the outstanding characteristic of mana is that, precisely because it is extremely powerful, it is regarded as transmissible and even contagious:

[In Polynesia] mana could be contained in any person or thing; however, a man's potential capacity depended on his position in the whole hierarchy. . . . since mana flowed from high to low, an unguarded contact between a chief and a commoner was therefore an evil thing; the chief suffered a loss of mana, . . . and the commoner . . . might be blown out like a fuse. [Howells 1962 (1948): 33–34]

The passage of valuable qualities from some sacred object—human or non-human, animate or inanimate, part or whole—to a less virtuous subject is often considered to be accomplished by the subject's touching that object. . . . In all such transactions, power passes from a donor to a receptor. This power may be generalized and omnifacile, as in *mana*. . . .

Just as, in some instances, sacred things are to be touched . . . so in other instances sacred things are to be not touched, or are otherwise to be avoided, in order to prevent the power that is in them from injuring the person. (Frazer lumped *mana* and taboo procedures in a common category of "contagious magic.") Such avoided objects are commonly said to be subject to "taboo." [Wallace 1966:60–61]

In terms of folk belief, then, mana is not comparable to psi, whose power is so faint as to require statistical analysis even to be discovered. Mana does not require belief. Its power is openly demonstrated: if one's garden grows well after a certain stone is buried in it, then that stone did indeed have mana (Howells 1962 [1948]:25). A better contemporary analogy to mana than psi would be "pyramid power," the general concept of which suggests that, just as a particular stone is filled with mana and thus can make one's garden grow, so the pyramid, filled with "pyramid power," makes the razor blade stay sharp or keeps fruit from rotting.

Probably it should also be noted that two entities that share some characteristics are not necessarily the same. The form of argument Winkelman uses amounts to the logical fallacy of the "undistributed middle." To be valid, such an argument would have to show that, besides sharing characteristics, the two entities somehow belong to the same class—and that is by no means established here.

Following Malinowski and Norbeck, Winkelman identifies the origin of magic as "spontaneous emotional forms, overflowing with passion and desire," as distinguished from "the institutionalized traditional mythological forms embodying social values and customs into which systems of magic develop." He then attempts to show that the first, earlier type of magic must involve psi, since psi is generally agreed to be involved—somehow—with emotion. But how, exactly? On this point, the research data of parapsychology are not only weak, but far more mutually contradictory than Winkelman's brief summaries reveal; as Hansel (1980:312) noted more generally, no "single lawful interrelation" is to be found.

For instance, Rogo (1975) does indeed review material some of which "implicate[s] emotional ties." Most of these cases suggest that the "ties" are strongly positive (e.g., pp. 122–26); some, however, seem to involve no tie whatsoever (e.g., pp. 127, 136, 140, 143–46).⁷ Rogo's chapter ends with a discussion

⁷ The effect of this evidence is diminished by Hansel's (1980:49, emphasis added) critique: "Members of the various societies who

of the Kappers Greiner study (pp. 146–47), in which no correlation between personality traits and spontaneous ESP experiences was found. Roll's work on poltergeists is considered by Winkelman to "suggest" that not emotional ties, but "a strong psychodynamic of repression underlies such phenomena." Ullman (1977), cited in apparent support of this point, in fact says (p. 567, emphasis added) that the work of Rogo and Roll *suggests* that "the implicated agent *may be* analogous to the identified patient in family therapy and like the latter may not be the source of the disturbance but rather, the victim. *Considerably more work would have to be done along these lines, however, before any of these conjectures can be defined convincingly.*" Ullman qualifies his more general point that "as people near psychotic breaks or crises they manifest much stronger psi events in the course of therapy" by noting (p. 557) that "in trying to relate psi events to psychopathology we face the uncomfortable fact that in neither instance are we dealing with clear-cut entities about which there is basic consensus." Finally, Dean and Nash (1967) are cited in support of the point that "the functioning of psi is closely related to emotional factors relevant to the individual's psychodynamics." As we have noted, however, the design of Dean and Nash's experiment is by no means conclusive. Furthermore, with regard to the psychodynamics, Stanford (1974:38) refers briefly to Devereux's (1953:40–41, 406–12) idea that instances of what seems to be ESP between patient and psychiatrist should be dealt with simply as part of the whole body of psychoanalytic data and also states (p. 52) another relevant point Winkelman does not mention: "While some studies have failed to show the anticipated significant negative relationship between neuroticism (and other indications of maladjustment) and ESP performance, in general the results of experimental studies point in this direction. There have been few significant reversals."

Thus we are left with questions: Does spontaneous ESP involve positive emotional relationships (or sometimes none at all), while poltergeist phenomena involve emotional repression? Are ESP abilities correlated with an approaching psychotic break, the patient's negative transference to the psychiatrist, or the absence of neurotic traits? Is ESP one thing in ordinary life and something totally different in laboratory experiments? It is at least evident that the ways in which "emotional tension" is "central to psi" remain to be discovered.

Winkelman then argues that both anthropological data and parapsychological research suggest "that psi processes are closely related to primary thought processes." Again, the hypothesis might be supported *if* we had good evidence for the existence of psi. What is clear, at least from the Western point of view, is that *belief* in magic represents a stage in the development of adult thought processes. R  heim (1962 [1955]:46) shows the symbolic nature of magical *thought* rather than the phenomenal reality of magical *events*:

We grow up through magic and in magic, and we can never outgrow the illusion of magic. Our first response to the frustrations of reality is magic; and without this belief in ourselves, in our own specific ability or magic, we cannot hold our own against the environment and against the superego.

The infant . . . learns in time to recognize the parents as those who determine its fate, but in magic it denies this dependency. The ultimate denial of dependency comes from the all-powerful sorcerer who acts out the role which he once attributed to the projected images.

R  heim explores the similarities between magic and schizophrenia: schizophrenics often claim to possess magical powers

have reported [spontaneous psi] experiences in journals must be aware of the importance of supplying supporting evidence, but, so far, these surveys *have failed to provide a single story that is conclusively supported by ample confirmatory data.*"

or to be influenced by others' magic (p. 94); their lack of ego boundaries means that they tend to identify with others and yet react with anxiety to any attempt to influence them (pp. 99–101); their fantasies often resemble effects claimed for black magic by the sorcerer and feared by the "normal primitive" (p. 102). He considers the major difference between magic and schizophrenia to be that, whereas in folk society magical processes "are generally ego-syntonic, and are dramatized and shared by the social group," in schizophrenic psychosis "these processes, though they are generally dramatized, are not ego-syntonic and are not shared by the social group" (pp. 226–27).

A closely similar point is made about psi by Reichbart (1978:169): "to grant reality to psi is to come face to face . . . with the possibility that our spontaneous, subconscious death wishes directed toward loved objects can be effective. This possibility is something which we do not wish to entertain." For this same reason, Reichbart says, watching stage magic is "analogous to watching a horror movie"; we react with excitement combined with fear and afterwards feel reassurance because what we saw was not real after all (pp. 169–70). Reichbart argues also that stage magicians often resolve the conflict between reality and "their wished-for omnipotence" by denial or reaction formation, which R  heim called (1962 [1955]:46) the denial of dependency: "magicians such as [Milbourne] Christopher and 'The Amazing Randi' . . . turn to magic in a kind of cynical parody of their former desire, and they deny that psi ability of *any kind* exists" (Reichbart 1978:171). Reichbart therefore suggests that "many parapsychologists, and others, who blithely invite magicians to comment on allegedly psychic events are extremely naive about the possible psychological depth of a magician's resistance to accepting parapsychological data no matter how convincing" (p. 171). Later he notes specifically "the transparent bias of Randi, who leaves Christopher far behind in his psychological need to deny the existence of psi" (p. 172)—a bias that we have also experienced (Singer and Ankenbrandt 1980:31; 1981). But it seems an inescapable corollary that the opposite attitude—uncritical acceptance of poor research design, questionable conclusions, and anecdotal "evidence"—just as clearly represents the mechanism of denial, though of the opposite variety.

Therefore, it seems to us that R  heim's work represents the "adequate theory of magic" that Winkelman says "must explain the role of psi in the development of magic and the reasons for and the process of generalization of the basic structures of magic into its social forms." The problem is not that this theory does not exist, but that Winkelman apparently is not acquainted with it. In the current state of the evidence, both parapsychological and anthropological, for the phenomenological reality of psi, a psychological explanation is quite sufficient. When, or if, any proof emerges, the theory can be enlarged to accommodate it.

For the most part, it does not seem necessary to comment on Winkelman's theory until, or unless, there is more hard evidence to support it. One point, however, should probably be made. Winkelman's discussion in this section of various theories of magic would seem to rule out culture-and-personality or psychological anthropology as a research field in favor of statistical manipulation of alleged psi events in a laboratory. In his discussion of "symbolist approaches," which "share a view of magic as a symbolic system which describes the pattern of social relations in the society in which it exists," he says that "symbolist approaches cannot account for universal magical practices and beliefs because universals cannot be explained as functions of highly heterogeneous social variables."

This point seems untenable: societies, however "heterogeneous," nevertheless exhibit common traits and themes. The psychoanalytic approach, as exemplified by R  heim's theory of magic, is a symbolic one in that R  heim treats magic as a code for the expression and communication of common human experiences involved in growing up and attempting to establish one's individual identity.

Finally, we would like to see some data—or even a connected argument—to support Winkelman's contention that "if magical practices and beliefs seem largely non-psi-related today, it may be because of a decline in the effectiveness of psi-related practices as a result of the disruption of the social institutions that once trained practitioners to manipulate psi potentials." It seems to us that, far from declining, such social institutions are multiplying today, in such forms as witch covens (Gundela and other self-described witches appear on national television), psychic readers, psychic and spiritual healers, psychic surgeons, and some forms of alternative medicine and psychotherapy (see Singer and Ankenbrandt 1981). College-level courses in parapsychology are taught at both our institutions of higher learning (see Wade 1973:139). There is the journal *Phoenix: New Directions in the Study of Man*, with a commitment to broadening anthropology to include psychic research, and its parent organization, the Association for Transpersonal Anthropology. There is the rack of tabloids beside the supermarket checkout stand, the *National Enquirer* prominent among them, that will seemingly print any story so long as it is mystical, magical, or occult. And in case psi ability turns out to correlate with mental illness, that also has an increasing incidence today—at this rate, large numbers of people should be near the threshold of psi events.

If they are not, there seem to be several possible explanations. Perhaps psi abilities do not really exist. Perhaps parapsychological research design has not been such as to demonstrate the real nature of such abilities. To investigate the question, we must go back to descriptive, "macro" approaches such as we have recommended earlier.

In conclusion:

1. We do not deny the possible phenomenological existence of psi abilities or events, but we do not consider that existence proven by the material that Winkelman cites or that we have thus far seen.

2. Similarly, we do not deny the possible phenomenological reality of magic, but we must insist on the necessity of testing claims to paranormal powers put forward by shamans, tent-shakers, diviners, water witches, swamis, psychic surgeons, and other religious specialists.

3. We do not think that, given the present state of the evidence for either psi or magic, psi phenomena can be employed to explain magic—or the reverse.

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Winkelman's very provocative theory that some magic works and that its primary, yet mysterious efficacy led to the invention of explanatory ontologies and cosmologies is more convincing when it is seen in a materialist light. This is, in part, what he seems to have in mind when he bases mental phenomena such as cosmologies on empirical psi events, but I think the materialistic nature of psi should be more forcefully delineated.

If psi phenomena possess empirical reality, they must be the product of some material activity, and anthropologists and parapsychologists must be able to explain eventually how psi works. The growing body of experimental, anecdotal, and field-observational evidence seems to attest to the validity of psi production. Now an amalgam with the neurosciences is needed

to generate the most fruitful hypotheses as to how psi works, what it is made of, and where it comes from. Few scholars who grant the reality of active psi phenomena would dispute that it is a product of the brain at work. Thus, to ground psi production in neurophysiological and/or neurochemical mechanisms is no more a reductionism than it is to acknowledge that dreams, poems, even philosophical inquiry are ultimately the product of biochemical events of the brain. In the brain materialism and mentalism are bound together.

Winkelman's conditions for the most conducive production of psi point to a biological substrate. Indeed, the most notable indication that psi is a neurological product is that it arises during an altered state of consciousness. Throughout the medical, psychological, psychopharmacological, and anthropological literature, altered states of consciousness are linked with structural, chemical, and/or electrical changes in the brain. These might include changes in brain wave activity (Wallace, Benson, and Wilson 1971, Blackmore 1977, Schuman 1980), differential glucose metabolism (*Science News* 1981), abnormal activity of discrete brain structures such as the reticular activating system (Moruzzi and Magoun 1949, Delafresnaye 1954), alterations of neurochemical synthesis (Cole and Katz 1964, Ray 1972, Furst 1976), or any number of other physiochemical changes. (Indeed, this list is extremely attenuated both for potential neuronal changes and for references regarding those changes.)

Since Winkelman draws distinctions between kinds of magic and the situations in which they occur, he should also distinguish between types of altered states of consciousness. These can be divided into hypoaroused (characterized by lower than normal somatic activity) and hyperaroused (characterized by higher than normal somatic activity). Goodman (1972) shows that these distinct types are induced by different "driving techniques." It is possible (but has not yet been tested to my knowledge) that they also produce distinct brain wave patterns, and the bifurcation may then appear in very different psi manifestations.

The second condition for psi production, visualization, should not be treated as a discrete potentiator, but rather as a tool that is sometimes used to generate particular altered states of consciousness. It should be noted, however, that in some instances visualization is deliberately eschewed (see Schuman 1980 for a discussion of the relations between visualization and hypoaroused trance). In these cases other driving techniques not mentioned by Winkelman, such as hyperventilation and stereotypic movements, are used to generate the physiochemical changes that are necessary precursors of altered states of consciousness.

If psi has a biological reality, it is capable of doing work; therefore the harnessing of psi through magic is a technology. This would allow us to understand why some magic is psi-related while other magic is not. Winkelman feels that non-psi magic is a degraded ritualistic by-product of the institutionalization of magic, but this is simply a matter of putting the question one step off. Merely explaining institutionalization as an expected outcome of the developmental sequence of magico-religious formations (Wallace 1965, Weber 1956 [1922]) avoids relating magical practices to the ever changing fabric of other cultural elements, especially other technologies. If magic is viewed as a technology in itself, its effectiveness can be compared to the production capabilities of alternate technologies, and the ensuing efficiency analysis can provide a rationale for the patterning of psi and non-psi magic among and within societies.

I would suspect that psi magic is a low-efficiency technology in relation to the technologies that herald industrialization. This would explain a generally decreasing reliance on magic. At the same time, the notion of magic as competing technology would explain (1) pockets of magical beliefs and practices

existing within a modernized society and (2) the institutionalization of magic as a form without primary-work content. In the first case, magical praxis might coexist with advanced technology because magic may work in those domains in which mechanical technologies have low efficiency. In the second case, whereas neither magic nor mechanical technologies are efficient, magic may be utilized in its institutionalized form because it offers secondary rewards (Malinowski 1948, Vogt 1952). When this is the case, magic is best studied in a mentalist framework.

Reply

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It is encouraging that the majority of the commentators recognize the importance of further investigation of the topics raised here. Clearly, there are differences of opinion with respect to the nature of the evidence accumulated to demonstrate that some magical practices involve manipulation of psi; however, ignoring the connection between the two has been detrimental to our understanding of these phenomena. My response focuses upon a few main issues: the notion of a new paradigm, the definition of magic, cultural factors shaping response to reports of psychokinesis and conceptualizations of psi and mana, the relationship between psychosomatic and psychokinetic effects, the reliability of parapsychological research, and, finally, misrepresentations of my work and that of others.

Several commentators suggest that the perspective offered here is a new paradigm. If a paradigm shift is occurring, however, it is at the level of the disciplinary matrix, not at the level of exemplars, where much of Kuhn's work focuses. Analysis of the relationship between science and parapsychology (Winkelman 1980, Mackenzie and Mackenzie 1980) suggests changes far more basic than those involved in what Kuhn refers to as a paradigm shift. I have argued elsewhere that these changes amount to what Popper (1975) calls an ideological revolution. Although Kuhn's work is of limited value in explicating changes at this level, it is apparent that his model does not necessarily imply impending scientific revolution, regardless of the quality of the evidence for psi. Scientific revolutions occur when anomalies conflict with the progress of the explication of a paradigm. "Normal science" can ignore anomalies as long as they do not impede the articulation of the paradigm in the directions dictated by the values and metaphysics of the scientific community, which do not change during scientific revolutions or paradigm shifts. Acceptance of parapsychological research depends less upon scientific evidence than upon social and cultural factors such as changes in the values of the scientific community.

As Romanucci-Ross points out, emulation of 19th-century physicists has been detrimental to exploration of the "paranormal," and a new methodology is needed. It appears that explaining psi requires fundamental changes in metaphysics, epistemology, and values as well. Feyerabend (1975) argues that the Copernican revolution required whole new sets of observations and evidences, new observational languages and means of observation, and new world views and frames of reference. In constructing a new system, we must keep in mind that any system of knowledge must start from untested and untestable assumptions (Popper 1959, Feyerabend 1975), basic axioms from which we derive consequences that we can verify or falsify. The new system we develop should not only address deliberate and obvious manifestations of psi, but also explore the possible role of unconscious psi in such activities as language acquisition and other forms of enculturation.

As Goodman, Hultkrantz, and Womack point out, paranormal activities involving altered states of consciousness seem an

ideal area for further investigation of psi abilities. Kelly and Locke (1981) confirm this in their historical survey. Womack suggests that we begin to acquire data about concomitant functioning at the neurophysiological level; the Experiential Learning Laboratory of the Department of Electrical Engineering at Duke University has taken a major step in this direction with the development of portable radio telemetry systems (Klein 1976) that can be used for monitoring physiological variables in the field. Locke and Kelly (1981) offer a framework for cross-cultural analysis of altered states and psi which could profitably guide both field research and reviews of ethnographic materials.

Dundes suggests the need to relate this discussion to "debates over the difficult question of the definition of magic (and the inevitably fruitless attempt to differentiate it from religion)." Such a project was beyond the scope of my paper and premature from the point of view taken. Definitions of magic have suffered from two assumptions brought into question here: that magic is a unitary complex and that it has no technical efficacy. The argument here is that what has been considered magic involves different phenomena with different ontologies and functions and that some of it is effective in ways we do not yet understand. More investigation of magic within this perspective will be necessary before proceeding with definitions and comparisons. A redefinition of magic within this perspective would, however, crosscut the typology offered by Rosengren (1976). The existence of psi-related magic as an example of an empirically effective technique would invalidate his typologies and substruction; social ritual and cosmological/metaphysical magic would probably fall within his "ceremony" and "ideology." Even if magic is empirically effective in some cases, it still must be understood as a world view (Wax and Wax 1963) and as a form of religion (Hammond 1970). In fact, the three aspects of magic suggested here would all appear to be present within religious activities. I deliberately avoided the relation of magic to religion and the implications of psi phenomena for studies of religion (although some religious writers have not; see, for example, Thurston 1952). It is interesting that none of the commentators has raised the issue either.

Hultkrantz, Hallpike, and Preston address the confusion of different aspects of magic with other processes. Clearly, magic does not always involve psi, nor is it always effective. The operation of "blocks to falsifiability" undoubtedly plays a much greater role in maintaining magical belief and practice than do psi-mediated successes. My suggestion is that magic (occasionally) involves psi effects, not that psi necessarily leads to magic. Clearly, human needs and other psychological and cognitive processes not directly related to psi provide the impetus for the development of magical practices. However, it does appear that magical practices have to some extent developed along lines recognized as facilitating the manifestation of psi. The similarity between the associational processes of the human mind and the principles of psi and the integration of magic with other technologies undoubtedly contribute to a confounding of the different processes in the development of magic and of the different aspects of magic. Failure or inability to verify successes or account for failures of magical practices would contribute to a confusion of various aspects of magic both by practitioners and by investigators. As Preston points out, broader research on how people perceive, experience, and report anomalies is necessary to guide the reformulations suggested here. The confusion may be unresolvable; the different aspects of magic identified here—psi-related, metaphysical/cosmological, and social ritual—should be seen as focal points, not as exclusive or exhaustive categories. The perspective offered here is exploratory, not final.

Preoperatory thought bears a certain resemblance to magical

beliefs and undoubtedly contributes to the difficulty in disentangling the various aspects of magic. However, instead of asserting that magic represents a stage in the development of adult thought processes we should say that we attribute magic-like principles to earlier stages of human thought development. There are very few continuities between the magical practices observed by anthropologists and the characteristics of the early stages of human thought that we call magical; the attribution seems somewhat ethnocentric. Additional research is necessary to clarify the extent to which the attribution of preoperative or magical thought to children and members of primitive societies is appropriate.

Dundes suggests "that whereas 'psi' involves *knowledge* of events, magic involves *causing* events." However, his list of "principal alleged manifestations of 'psi'" omits psychokinesis. Although I agree that "magic in the *more rigorous, narrow sense of the analytic term* does imply the influencing or manipulating of nature in some causal way" (emphasis added), a broader definition would include divination. Perhaps Dundes's point is that magic in the narrow sense needs to be differentiated from divination. With this I completely agree, and I implicitly recognize these differences in organizing the sections "Magic and Psychokinesis" and "Divination and Psi-Mediated Information Gathering." Although parapsychological research may be more germane to divination than to magic narrowly defined, as Dundes suggests, it remains to be seen whether PK effects can be shown, under experimental controls, to be involved in some magical acts.

The comments of Dundes and others (e.g., Romanucci-Ross, Hultkrantz, and Bharati [1977]) indicate more resistance to the idea of PK than to that of ESP, a resistance that seems to have a broad cultural basis. The Society for Psychical Research in general early abandoned studies of physical mediumship in favor of studies of mental mediumship, although very impressive physical phenomena were obtained by skeptical investigators skilled in detecting fraud and working under controlled conditions (Fielding, Baggally, and Carrington 1963 [1908]). D. D. Homes also provided excellent evidence for macro-PK under laboratory conditions for the physicist Crookes (1874), who came to psychical research with the intent to debunk what he considered fraud. Rhine delayed publication of his first PK research for a number of years, recognizing the tenuous acceptance of ESP research and the greater resistance to PK. Some of the skepticism of the physical phenomena is based on the greater possibility of fraud. However, as Eisenbud suggests here and explores more fully elsewhere (Eisenbud 1970), guilt about the destructive aspects of psi has resulted in repression and projection and resistance to the idea of PK may be related to these processes.

Romanucci-Ross points out that in Melanesia *mana* is part of the "regular course of nature." Since my discussion of *mana* focused upon it as a "universal or near-universal concept" as discussed among anthropologists, divergences from the Melanesian concept could be expected. However, I agree with her. The definition of *mana* is a product of the Western mind encountering an alien concept; psi was originally defined in direct opposition to the dominant materialist/mechanistic framework of Western science, and it is this definition, rather than the contemporary ones reflecting a more operationalized perspective, that I adopted. There is undoubtedly misrepresentation of the domains to which psi and *mana* apply in such a way as to produce some spurious congruence. Rao (1979) and Harary (1981) suggest that psi is an aspect of nature, as Romanucci-Ross suggests with respect to *mana*. Nonetheless, there are congruences between the concepts. I am not arguing, as Singer and Ankenbrandt suggest, that psi and *mana* are the same, but rather that, although they differ, they show some fundamental similarities as a result of being formulated in relation to similar domains.

Sebald requests clarification of the relationship between

psychosomatic and PK influences. The concepts have similar bases, mind over body and mind over matter respectively. The primary difference seems to be that while the psychosomatic influence ends at the boundary of the body to which the mind is attributed, the PK influence implies no spatial limit. Psychosomatic effects could be considered a special class of PK effects. Although the majority theoretical viewpoint in neuroscience is that there is a one-to-one relationship between physical and mental events, some eminent brain scientists (e.g., Sherrington 1955, Penfield 1975, and Eccles [Popper and Eccles 1977]) favor an interactionist point of view in which some forms of human activity cannot be explained in terms of brain functions alone. Eccles's hypothesis that the self-conscious mind interacts with critically poised regions of the brain, "effect[ing] changes in neuronal events" (p. 363) "to deflect and mould dynamic patterns of activities" (p. 364), implies a PK-like force, although he and Popper "decide not to refer to parapsychology" (p. ix). The interactionist position requires some mechanism through which the mind interacts with the brain, presumably something akin to a PK force, and seems to require an assimilation of the psychosomatic to the psychokinetic.

Whether or not one accepts the dualistic position, some problems in distinguishing the psychokinetic from the psychosomatic do arise. Although controls such as isolation of the healer from the patient and randomly and blindly counter-balanced treatment periods help eliminate the possibility of gross psychosomatic responses, problems remain. If ESP operates, what appears to be a PK effect might be a psychosomatic response of the patient initiated by his/her unconsciously extrasensorially acquired knowledge of the healer's intent. Monitoring the physiological states of healer and patient may reveal details of the interactive process in such a way as to make PK the more likely explanation, but it may remain impossible to distinguish the two. For this reason, the establishment of psi effects in healing ceremonies is problematic. Elimination of the possibility of gross psychosomatic effects would seem to involve such drastic alterations of the interactive context that the experimental design would violate its integrity. Failure to provide evidence for psi under such conditions would not tell us about psi effects in the normal healing context. Descriptive studies may eventually enable us to make some distinction between the two processes in the field. As Goodman (1974) suggests, effects with infants or moribund patients reduce the plausibility of psychosomatic effects, as would the use of animals as patients. Since we may not be able to establish psi conclusively in the field without destroying the context which apparently creates the manifestations, we may be forced to rely upon laboratory studies to establish the general characteristics of psi and then use those characteristics to direct us to those aspects of activities which appear to involve it.

As Long points out, we should not expect ethnography to establish psi with the same level of reliability as we find in the laboratory. However, we cannot limit our investigations to those phenomena with which we can establish the operation of psi with absolute certainty. Although descriptive evidence is of little value in convincing skeptics, it is essential for understanding the phenomena. As Dobkin de Rios points out, we need collections of descriptive studies of events which apparently involve psi to identify the conditions and circumstances favoring such events. Case studies can be invaluable in this regard (e.g., see Rhine 1981). As Fontenrose (1978) demonstrates in his analysis of the accounts of the oracles of Delphi, such collections can provide their own internal criteria for differentiating valid and invalid cases. (Fontenrose does not suggest that the oracles are evidence for psi.)

Of course, we should apply experimental controls and instrumentation whenever possible. However, maintaining the context which facilitates or elicits psi may require that we utilize observation with controls rather than experimental paradigms. The use of cameras and magicians as controls suggested

by Singer and Ankenbrandt will not solve the problems raised by critics. Film provides very little convincing evidence, as Phillips and Shafer's workshop presentation of a film of ostensible macro-PK events at the 1981 Parapsychological Convention showed, and Randi's earlier film indicated the unreliability of film in revealing fraud. Nor is film likely to be of much evidential value in investigation of most aspects of magical practices, for example, establishing what extrasensory content may be present in the ecstatic experiences of shamans, in mediumistic trance, or in divination procedures or indicating whether or not PK is operating in most magical acts. Unless we test the psi hypothesis under controlled conditions, it will be possible to discount any potential psi phenomenon with labels such as delusion, hallucination, faulty perception, folklore, fraud, and placebo and psychosomatic effects. Acceptance of the psi hypothesis or the basic assumption of magical belief does not eliminate the need for further explanation. Rather, as Locke and Kelly (1981) demonstrate, it provides the basis for a systematic phenomenological investigation of the relationship between psi and other variables such as cultural beliefs and altered states of consciousness.

The reliability of parapsychological research is a principal concern in considering the implications of parapsychology for anthropology. Jarvie's suggestion that work on stage magicians, the Bermuda Triangle, and Castaneda attests to the "delusion and fraud" of the "logic and methodology of psi" is an example of the rejection of parapsychology on the basis of material that has nothing whatsoever to do with it. Singer and Ankenbrandt's criticisms appear more closely tied to parapsychological research but are vitiated by heavy reliance upon Hansel (1980). Child (1980) argues that Hansel's book is propagandistic, repeats many assertions shown in print to be erroneous, and demonstrates ignorance of important publications in the parapsychological literature. Hansel's (and Singer and Ankenbrandt's) biases are revealed when one notices that the works considered were published 20–40 or more years ago.

By the criteria of replication, parapsychology does extremely well with respect to other aspects of science. Honorton (1976) reviews English-language publications between 1934 and 1939, the period immediately following Rhine's initial publications and the one in which academic psychology paid the most attention to parapsychology. He finds that of 50 studies, 70% were replications, including 61% of the 33 studies carried out at independent laboratories. He compares these figures with Sterling's (1959) report on a sample of 362 published research reports in psychology, none of which were replications. He also cites Bozarth and Robert's (1972) examination of 1,344 psychological research reports, of which fewer than 1% were replications. The Parapsychological Association conference to which Honorton was reporting included 48 experimental reports, 73% of which were attempts at replication of previous findings, 43% of them successful.

Singer and Ankenbrandt relay Hansel's assertion that the criteria for conclusiveness in parapsychological research have not been met but fail to deal directly with what Hansel sees as its crucial shortcoming—insurance against experimenter fraud. Since critics refuse to credit parapsychologists with personal and scientific integrity, meeting their criteria of conclusiveness is problematic. Recognizing the impossibility of excluding experimenter fraud (in any branch of science), parapsychologists have formulated new criteria for the evaluation of their data. The idea of the conclusive study has been recognized as unrealistic; parapsychologists recognize that individual studies can never be immune to criticism in the form of any number of unfalsifiable hypotheses. Morris (1980b) suggests that, in general, exact replication is impossible, especially since subject characteristics and handling are crucial and nearly impossible to maintain constant between studies. He argues that the important criterion for assessing parapsychological research is the extent of general conceptual replication within certain areas of

research, particularly those which relate psi scoring to some other variable. He suggests that studies relating variables such as absence of neurosis, extraversion, hypnosis, and avoidance of response bias to ESP success (reviewed in Palmer 1978:sect. 3.2.1, 3.2.2, 2.6.1, and 4.1.2, respectively) constitute a consistent set of findings. Other areas of partially repeatable research include findings that hypnagogic-like states are psi-conducive (Palmer 1978:sect. 2.6.2) and random-event generator PK studies (see Schmeidler 1977:sect. 4.4 for review). Effective criticism of parapsychology would require the posing of alternative testable hypotheses for the relationships found. Studies which relate psi to some other variable would appear to be particularly immune to criticism for subject fraud, since one would have to assume that only persons displaying the variable were able to perpetrate fraud.

Many of Singer and Ankenbrandt's criticisms are directed at misrepresentations of my paper; a careful reading will differentiate what I state from what they attribute. A point-by-point response to these is impossible within the space available. Given their concern with the evidential value of citations, it is curious that they fail to apply similar criteria in the selection and evaluation of the materials they cite. A number of their criticisms are based on misrepresentations of others' work. One is reference to Hansel's accusation that the Pratt-Pearce series could have involved subject fraud; Stevenson (1967) reviews many of the published responses to Hansel's (1969) inaccurate accusations, including detailing the physical impossibility of the scenarios Hansel suggests. (Hansel's updated version fails to correct these inaccuracies.) Singer and Ankenbrandt cite Hansel regarding Marks and Kammann's (1978) claim that the judges in Targ and Puthoff's (1977) study received the list of targets in the same order in which the targets were visited; Morris (1980b) has since pointed out that the researchers provided Marks and Kammann, during the draft stages of their book, with additional details correcting the scenario suggested.

Singer and Ankenbrandt distort Murphy's (1971) work in asserting that he says there have been no satisfactory demonstrations of ESP and no lawful interrelations in parapsychology. Actually, Murphy is considering "laws comprising generalizations about conditions under which extrasensory perception occurs" (p. 5) and does not at all call into the question the evidence for ESP itself. Furthermore, he points to "three widely quoted parapsychological laws," including "a rather striking series of successful replications, showing those who believe in the possibility of success in clairvoyance in experimental conditions have scores that run above chance expectation" (p. 5), and says that this factor has "been rather well replicated in the majority of studies." Murphy does not deny that there are regular interrelations, but points out the failure to state and test "psychological laws . . . in a firm, clear, quantitative form" (p. 8, emphasis added), such as the conservation and gas laws of physics. Although Murphy does point out the need for a more consistent repeatability, he suggests that some generalizations can be made.

Singer and Ankenbrandt link in a single sentence references to criticism of parapsychological research designs and a recent article on fraud in science, without pointing out that the article on fraud makes no reference whatsoever to parapsychology. Although they "rule out the possibility" that Levi (1979) is a replication of Morris et al. (1979), since there were several minor differences, they assert that Marks and Kammann failed to replicate Targ and Puthoff's work without considering the major procedural differences in Marks and Kammann's attempt. With reference to Grad's (1963, 1964) research, they suggest that there was a failure to control the experiment adequately because of the role that the healer played as a co-experimenter, but they fail to mention that blind and double-

blind procedures were employed to eliminate the possibility that any sensory information or bias could cause spurious findings.

Singer and Ankenbrandt suggest that the notion that fraud and trickery may facilitate psi is difficult to reconcile with the material I cite, but they ignore what that material actually claims. Although Reichbart does not attempt to establish the reality of shamans' psi abilities, his thesis is that "magic [trickery] is used by shamans as a psi conducive technique . . . which I would suggest actually works given the proper circumstances" (p. 156; also p. 157 and elsewhere). Although Lévi-Strauss (1963a) is discussing the role of trickery in a social psychological framework, he makes it clear that the shaman recognizes that successful sleight-of-hand leads to cure and employs it deliberately. Rose points out (pp. 94-95) that the Aboriginal doctors recognize that trickery helps the patient get better, and although he considers hypnosis as a hypothesis he is very critical of its usefulness: "Hypnosis can be invoked to explain many aspects of aboriginal magic, including fast traveling. By doing so, however, we place extraordinarily heavy demands upon it and some explanations seem rather far-fetched" (p. 168). Even when Rose does suggest hypnosis as a mechanism, he is clearly talking about something more than hypnosis as conventionally understood. Invoking hypnosis as an explanation of apparent psi events is of little use when the processes involved are as incredible and as little understood as those of psi (and presumably may involve it). Although Rose does suggest that some of the claims to paranormal powers are based on exaggeration, myth-making, and mass hypnosis, he also states that "the conditions favoring psychic awareness are probably optimum in the case of the aborigines" (p. 156) and that "telepathy . . . is the least far-fetched hypothesis" (p. 155).

Singer and Ankenbrandt call inaccurate my summary of Ullman's (1977) article in which I suggest that "Ullman (1977) points out that as people near psychotic breaks or crises they manifest stronger psi events in the course of therapy." This was not a summary, but a reference: according to Ullman (1977:563), "Ullman (1949, 1952), in his clinical observation, noted that patients who function close enough to a psychotic break to be aware of its possible imminence do manifest psi ability in the therapeutic context more frequently and more consistently than do other patients." The point is certainly subject to debate, but it is not debatable that Ullman makes it. The quotation Singer and Ankenbrandt provide suggesting that Ullman qualifies this point is from the introduction to the article and is not directed at this generalization.

In referring to the materials which suggest that poltergeist cases involve a strong dynamics of repression, Singer and Ankenbrandt point out, citing Ullman, that the work of Roll and Rogo indicates that the agent may be a victim rather than the source. Rogo (1980) recognizes that poltergeist cases may reflect a family dynamics, with all members showing high levels of repression and aggression, but this does not contradict the point that repression is present. Singer and Ankenbrandt discuss other portions of the work relating psi to emotional factors and seem to imply that the additional sources they provide contradict my position. In fact these citations (e.g., Stanford 1974) support it. Further support comes from the findings that extraversion and absence of neurosis are positively related to ESP (see above) and that defensiveness inhibits it (see Haraldsson and Johnson 1979 for review). Singer and Ankenbrandt point out that the ways in which emotional factors relate to psi remain to be discovered; the need for further investigation does not refute the point that such relationships are central. In addition to these, there are links between emotional factors and other psi-conductive factors such as altered states of consciousness. As Honorton (1977) points out with reference to the work on altered states, these findings suggest support for the general theoretical perspective provided by Bergson's filter

model, in which decreases in barriers in the organism (such as decreased defensiveness or neuroticism or the changes brought about by psychotic breaks or altered states of consciousness) facilitate ESP.

Experimental findings suggest that enculturation experiences influence the manifestation of psi abilities. Such findings need to be subjected to cross-cultural verification; ethnographic investigations are indispensable for the explication of the relationship of psi to culture. Other foci of future research would include examination of the effects of child-rearing practices and life-styles upon psi abilities and the ways in which social forces mold the personalities of paranormal practitioners.

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